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THE QUARTERLY JOURNAL OF
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Vol. XVII 1965 Part 1

ADAPTATION AND REPULSION IN THE FIGURAL AFTER-EFFECT

BY

J. WILSON

From National Physical Laboratory, Teddington

An experiment is reported which shows the effect of inspection of a curved line on the apparent curvature of a curved test line for a range of curvatures of both I and T lines. A second experiment extends the range of I curvatures, using only one T line, a straight line. The experiments showed both adaptation and repulsion components in the FAE.

An experiment by Köhler and Wallach which could not be reconciled with these results was repeated in the relevant part; the results were in agreement with the first experiment here and did not agree with those obtained by Köhler and Wallach.

An argument is presented that both adaptation and repulsion effects could be produced by a cell adaptation mechanism.

INTRODUCTION

Two concepts have guided work on the figural after-effect: (*a*) adaptation to a norm, not strictly a physiological adaptation, and (*b*) displacement of one contour by another, a displacement which is always "away from" and which therefore can be described as a repulsion. Adaptation represents the view of Gibson (1933, 1937); in this view, after-effects in the perception of curvature, tilt, etc., are a result of changes in the subject's norms for the corresponding dimensions of perception, changes exactly analogous to the physiological adaptation which occurs when for example the skin senses adapt to warmth. In the opposing point of view, put forward first by Köhler and Wallach (1944), the apparent displacement of contours seen in the after-effects is the result of a displacement of activity in the cortex away from the traces left by earlier activity; they allowed that adaptation of the sort which Gibson proposed might be present, but their own experiments showed that it could not be important.

The experiments reported here were undertaken because a definite decision between these concepts could not be made on the basis of published experiments and because the after-effect is an important feature of perception, perhaps connected with perceptual learning. From our results it is clear that neither concept by itself is sufficient to explain the after-effect, which seems to have components of both. In our view, however, there is no antagonism between the two concepts and we have endeavoured to show that both may be derived from the same mechanism.

In Gibson's experiments the subject inspected a curved line (a flexible rod) for a few minutes. The line seemed gradually to become less curved. The subject then attempted to straighten the line, but made an error which left the line still curved though it appeared to be straight; a straight line would appear to be curved the other way. The after-effect seemed to act most strongly on lines in the same retinal position and orientation; it was large at first, and faded away gradually; similar effects were seen with bent lines, tilted lines, and in the kinaesthetic mode by moving the hand for some time on a curved edge, then testing on a straight edge. The results of all these experiments accorded with Gibson's view that the visual system adapts to curvature, tilt, etc., as the skin adapts to warmth. The consequence would appear to be that after long exposure the curvature, tilt, etc., would seem less intense and a stimulus previously neutral would now show the opposite curvature, tilt, etc.

Köhler and Wallach came to their study of the after-effect from their work with ambiguous figures, and this influenced their approach. From their explanation of the changes seen in ambiguous figures came the suggestion that the presence of a figure in a given location tends to operate against further presence of this figure in the same place because, in the visual sector of the brain, a "figure process" which governs the perception of the figure, provokes a build-up of resistance to the continuing of the "figure process" and hence also to the continuing perception of the figure. (This idea was developed by the authors into their well-known theory of cortical satiation, but the details of this theory do not concern us here. For an exposition and review of the after-effect, see McEwan, (1958).)

If this were true not only for ambiguous figures but for any object of vision, then after-effects, like those studied by Gibson, were to be expected. According to Köhler and Wallach, however, these after-effects were the result not of adaptation but of repulsion, the "test figure process" being displaced by the traces of the "inspection figure process." Köhler and Wallach experimented with a great variety of figures, and they obtained results which strengthened their view that there was an apparent repulsion between contours of the inspection figure and contours of the succeeding test figure. For example (their Fig. 5), the inspection figure was a circle with a fixation mark below it, the test figure was a pair of equal squares smaller than the circle, one above and one below the fixation mark. After inspection, the upper square appeared to be smaller than the lower, as if the contours of the square had been driven away from the position previously occupied by the circle.

Köhler and Wallach pointed out that it is easy to imagine that there is a norm for curvature or for tilt, but difficult to specify the norm for the figures they were using. No doubt the norm would have some influence, but the determining factor was the relation between I and T figures. If adaptation were important, then a symmetrical I figure, which clearly must represent a norm, would produce no after-effect, yet their experiments showed such effects. To emphasize their point, they next performed an experiment with tilted lines (described further on) which showed that inspection of a vertical line (the norm) changed the apparent tilt of a sloping line by the same amount as inspection of a sloping line changed the apparent tilt of a vertical line. This complete symmetry showed that adaptation was not important, that repulsion between contours was enough to determine the figural after-effect. Thus their theory was designed to explain only the repulsion. In this, Köhler and Wallach seemed to disregard Gibson's observation that apparent curvature decreases with continued inspection.

Experimental results obtained by later workers supported Gibson. When Fox (1951) attempted to measure a repulsion effect he obtained results reflecting an asymmetry in the figure; he made careful measurements with other figures, and found adaptation as well as the repulsion he had set out to measure. Nosawa (1953) obtained results which clearly showed an adaptation effect and seemed to exclude repulsion. His subjects were required to inspect a curved I figure, then match a flexible T curve to a comparison curve seen on a less affected part of the retina. He found that apparent curvature of T was always reduced even when T was more curved than I. This result was all the more significant because the use of such comparison curves had been demanded by Köhler and Wallach when criticizing Gibson's result.

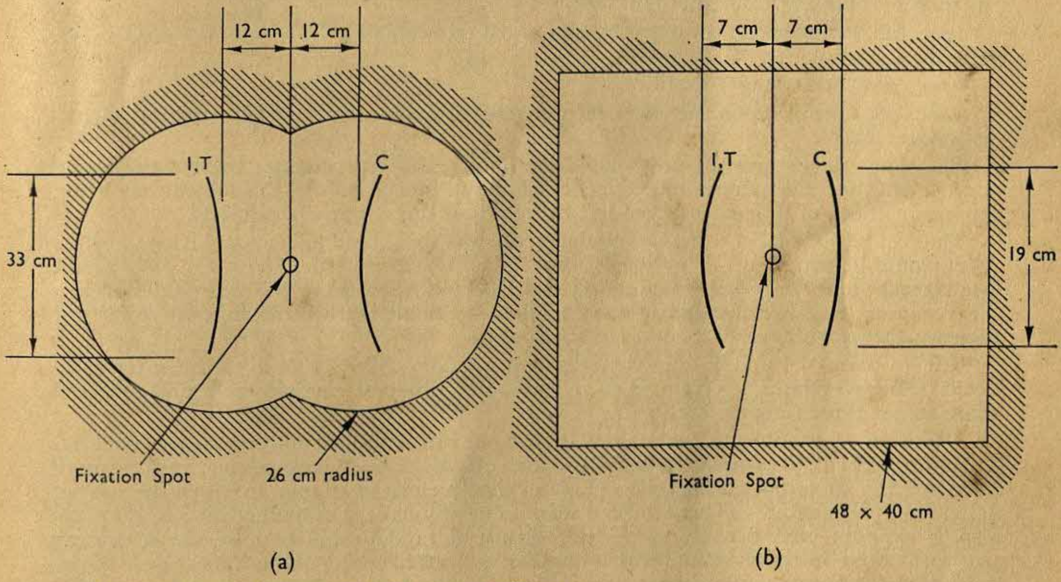
We now come to our own experiments, which were designed to show the variation of after-effect over a range of both I and T figures, using curvature as the variable of interest. By covering a wide range it was our intention to put the earlier results into perspective, and to show if and where adaptation, repulsion, or both, occur; we have avoided the danger of choosing conditions which exclude one or the other, and have included conditions in which we can expect to see the transition from one to the other. In the event, the results obtained could not be reconciled with those obtained by Köhler and Wallach in their tilted line experiment mentioned above, and it was necessary to repeat the relevant part of their experiment.

Experiment I

This experiment was required to show the figural after-effect, using curved lines for both I and T figures, over a range of curvatures for both I and T.

The lines, shown in Figure 1*a*, were projected on a white screen in a dimly lit room. The subject sat with his eyes 1·6 m. from the screen, keeping them fixed on a spot 3 mm. diameter, lit from behind the screen. The curved I line was 33 cm. long, vertically oriented and placed 12 cm. to the left of the fixation spot; for test, a T line replaced the I line and a comparison line C was projected 12 cm. to the right of the fixation spot in order to fall on a relatively unaffected part of the retina, as was demanded by Köhler and Wallach and used by Nosawa. The lines were dark on a bright field. To ensure that the edges of the field did not interfere with the measurements, its size, 26 cm. radius as shown in the diagram, was chosen on the basis of preliminary experiments. The figure, field and lines, was projected by a two-beam tachistoscope, one beam serving for the left side of the figure and the other for the right.

FIGURE 1



(a) (b)

Dimensions of figures used in the experiments.

(*a*) For small range of I curvatures, Experiment I.

(*b*) For large range of I curvatures, Experiment II.

For inspection a curved I line is shown and the right-hand half of the field is left blank. For test a curved T line replaces the I line and a comparison line C is put in the right-hand place. The subject judges the symmetry of the figure made by T and C lines. For successive judgements the curvature of C is varied.

Eight I lines were used, curved so that the ends were 0·56, 1·7, 2·8, 3·9 mm. left and right from the straight. There were five T lines, one straight and the others curved, 1·7 and 3·4 mm. left and right from the straight. The eighteen comparison lines covered a range from 9·35 mm. left to 9·35 mm. right in steps of 1·1 mm. In the experiment, the apparent curvature of the T line was determined by asking the subject to compare it with a C line. From preliminary tests, it appeared that a more reliable judgement of equality was obtained if T and C were curved in opposite directions, and the subject was instructed to judge both lines as a whole, that is to say, to judge whether the composite T and C figure was curved "concave left" or "concave right." For example, if the appearance of the figure was such that the top and bottom were apparently to the left of centre, it was judged "concave left." The subject was told to make a decision one way or the other, only the two categories of concave left and concave right being allowed. At times the subject seemed to use some other, perhaps conflicting, criterion, e.g. curvature of the single T line, for his judgements of right or left. His judgements then were erratic or reversed. When this happened, the session was adjourned, and usually the difficulty did not recur.

To show the figural after-effect over the whole range of curvatures, the apparent curvature of T must be measured after inspection of I for every possible pair of I and T lines, that is to say we shall have forty pairs for each of which the measurement must be made. For each pair, this can be done by placing a line C in a relatively unaffected region and asking the subject to compare T with C. By using a different curvature of C at each judgement according to the method of serial exploration, the apparent curvature of T is established.

A number of judgements were combined into a run of about 10 min. The timing of the tachistoscope beams during the run was controlled by a clock, the experimenter setting the films and recording results in time with the beams. One run is as follows:—

The run is concerned with only one I; the experimenter sets this. The subject is seated, looks at the fixation point, and the timer is started.

15 sec. inspection, beam one projecting the left half of the figure with I, beam two projecting a blank field for the right half.

4 sec. off, during which the experimenter sets T on beam one, C on beam two. (T is the same for the whole run, C is varied.)

1 sec. test, projecting T and C.

4 sec. off, during which the experimenter resets I and a blank, and the subject makes his judgement.

The timer then starts the next inspection period, the experimenter records the frame number of C and the subject's judgement. The run proceeds until the subject has made three transitions of judgement from left to right and three from right to left.

The rest period of 4 sec. between judgements was short, and an accumulation of after-effect would be expected. We thought that it could be disregarded, however, as it would be a second order effect and would in any case affect all the runs in the same way. When the accumulation was checked it was found to be small, with no significant difference between the first and second halves of the runs.

The 40 pairs of I and T lines were taken in random order for each of the subjects to avoid possible systematic errors; then repeated in a different random order and repeated again to get three runs for each pair.

There were two subjects, each taking in turn one run as subject and one as experimenter. This meant that the experiment could conveniently be done by two people, with a run of about 10 min. as subject being followed by an effective rest of 10 min. in which the subject acted as experimenter. (There did not seem to be any danger in having the subject serve also as experimenter, because points were taken at random and because the laboratory work was done in terms of film frame number and not in terms of line curvature. Thus, neither subject nor experimenter knew at the time what result to expect from a run. Moreover, because the exploration series included repeats, the subject did not know his position in the series.) Only two runs were made by one subject in one session, and not more than eight in a day; the experiment being spread over 2 months.

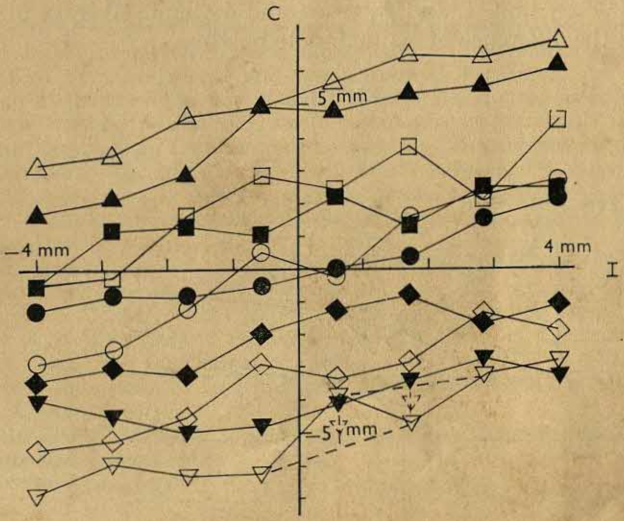
The procedure described above measures the apparent curvature of the T lines after inspection of I. In order to establish the amount of after-effect, it is necessary to measure the apparent curvature without previous inspection of I. The same procedure as above was used, but the runs were shortened by the omission of the inspection period.

Results

The result is shown in Figure 2 and Figure 3. Figure 2 shows the separate determinations of apparent curvature for the two subjects, one represented by open points, the other by filled points. Each point represents apparent curvature C for a given pair of I and T lines for one subject; the curvature C is given as the average of six transitions on three runs, 18 transitions in all.

These results can be seen more clearly in Figure 3, which shows smoothed curves of C against I for each of the five T lines. These curves were obtained from Figure 2 by averaging the curvature for adjacent points in overlapping groups of three, giving half weight to the outer points of each group; this is shown graphically in dotted lines at the bottom of Figure 2. The smoothed curves for the two subjects were similar and were combined to give the curves shown.

FIGURE 2



Apparent curvature C of curved test lines after inspection of curved I lines (results of Experiment I). Two subjects distinguished by open and filled markers.

Each point represents an average of three runs for one subject with the same curvature of I and T lines. The abscissa represents the curvature of the I line, the ordinate represents the average curvature of C for symmetry with the given T line.

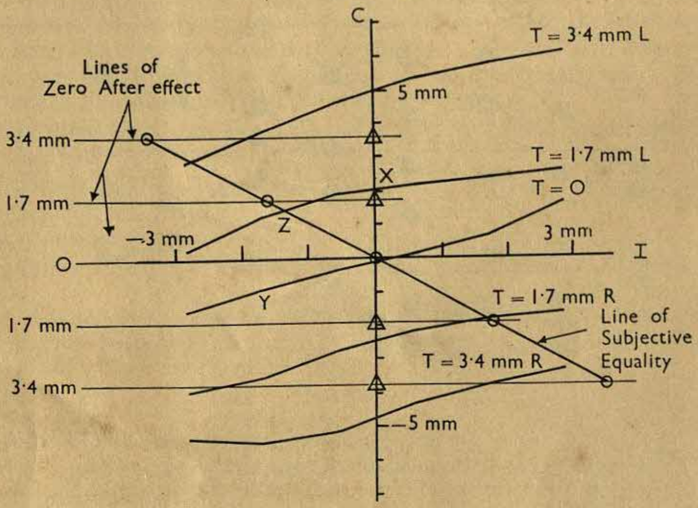
T = 3·4 mm. L; 1·7 mm. L; Straight; 1·7 mm. R; 3·4 mm. R.

Each of the plotted curves in Figure 3 represents the apparent curvature of one T line after inspection of the various I lines. To read the curve as representing after-effect it is necessary to establish for each T a horizontal line, the line of no after-effect for that T. From the measurements of apparent curvature without inspection we obtain points which define the diagonal line, the line of subjective equality between T and C. This line determines for each T a value of C which, when marked off, gives the required line of no after-effect. The deviation of the curve from that line then represents after-effect on a given T for various I.

The curves show that both the adaptation effect and the repulsion effect are present. If only adaptation were acting, inspection of a straight line would produce no after-effect, and the plotted curves would pass through the points marked by triangles. But each of the curves passes the corresponding triangle on the side remote from the origin, showing that apparent curvature of T has been increased and therefore that the repulsion effect is acting. If only repulsion were acting, there would be no after-effect when I and T coincide, and the curves would therefore pass through the points marked with circles. Instead they pass closer to the origin, showing an adaptation effect.

We can relate our results to the tilted line experiment of Köhler and Wallach, mentioned in the Introduction, by deriving from the curves of Figure 3 a comparison between the effect of straight I on curved T and the effect of curved I on straight T (Points X and Y on Fig. 3). If there were no adaptation the effects would be equal, as Köhler and Wallach argue that they should be. For I straight, we get, from points X on the curves for T = 1·7 mm. L and R, after-effects of 0·3 and 0·5 mm., average 0·4 mm. For I = 1·7 mm. L and R, and T straight, we get, from points Y, after-effects of 0·7 and 0·9 mm., average 0·8 mm. The difference of 0·4 mm. gives the amount of adaptation. The amount of adaptation read directly from the curves, at Z, when T = I = 1·7 mm. L and R, is 0·1 and 0·5 mm., average 0·3 mm., in fair agreement with the difference of 0·4 mm. noted above.

FIGURE 3



Apparent curvature C of curved T lines after inspection of curved I lines. These are smoothed curves for two subjects derived from the points of Figure 2. The abscissa represents the curvature of the I line, the ordinate represents the curvature of the C line for symmetry with the given T line. The horizontal lines are derived from the sloping line of subjective equality, itself derived from tests made without inspection of I, thus the average amount of after-effect for each I and T is shown by the difference in ordinate between the smoothed curve and the horizontal line for that T. For example:— I curved 2 mm. left (i.e.—2 mm.), on the curve for T = 1·7 mm. left, there is a displacement towards the origin, thus T appears less curved than it did without inspection. This could be an effect of adaptation or repulsion or both.

A similar result can be obtained from the curves for T = 3·4 mm. L and R, but because the ends of the curves were lost in smoothing an extrapolation must be made. For I straight, T = 3·4 mm. L and R, we get after-effects of 1·0 and 1·3, average 1·2 mm. For I = 3·4 mm. L and R, and T straight, we get after-effects of 2·0 and 1·9, average 2·0 mm. The difference of 0·8 mm. is the amount of adaptation. Read directly from the curves, as before, the amount of adaptation is 0·7 and 1·2, average 1·0 mm., again in fair agreement. There would thus appear to be a conflict between our results and those of Köhler and Wallach, who found no adaptation.

Experiment II

The I figures of Experiment 1 were chosen so as to cover only the central part of the range of the figural after-effect, that part in which the effect continues to increase with increasing difference between I and T. It is however a characteristic of the displacement effects studied by Köhler and Wallach that the effects reach a maximum and then decrease as the difference between I and T figures increases. To show that this "distance paradox" applies also to the curved line situation taken from Gibson, it was necessary to extend the range of measurements for at least one T line of Experiment 1. The range of I curvatures was therefore increased and the effect on the apparent curvature of a straight T line was measured.

The 21 I lines covered an extended range of curvature from 45 mm. left to 45 mm. right in steps of 4·5 mm. The T line was straight, and the C lines were from 3·12 mm. left to 3·12 mm. right in steps of 0·24 mm. A smaller figure than that of Experiment 1 was used, with lines 19 cm. long. (The change was made necessary by a modification of the tachistoscope for work unrelated to these experiments.) In preliminary experiments the figure was kept to the same shape and proportions as Figure 1*a*, but the experiments yielded a curve with kinks which were thought to be irrelevant, due probably to the lining up of the sharply curved I lines with the circular edge of the illuminated field. This difficulty had not occurred in Experiment 1 because the lines there were near straight and no connection with the edge could be perceived by the subject. To remove this source of interference a large rectangular field was used, as shown in Figure 1*b*.

The subject made his judgements as in Experiment 1, but another method was used to find the apparent curvature, because the range of measurement was greater. Each run was started as a serial exploration, which enabled the experimenter to find the approximate apparent curvature. Without stopping the run, the experimenter then chose a suitable range of six C lines, which were then presented in a frequency method. After each inspection of the I line, one of the six C lines was presented, the lines being chosen at random until each line had been shown six times. There were thus 36 inspection periods each followed by a judgement. The timing of exposures and all other conditions were the same as in Experiment 1.

There were two subjects, one of whom had served in Experiment 1; as before, each served as subject and experimenter in turn.

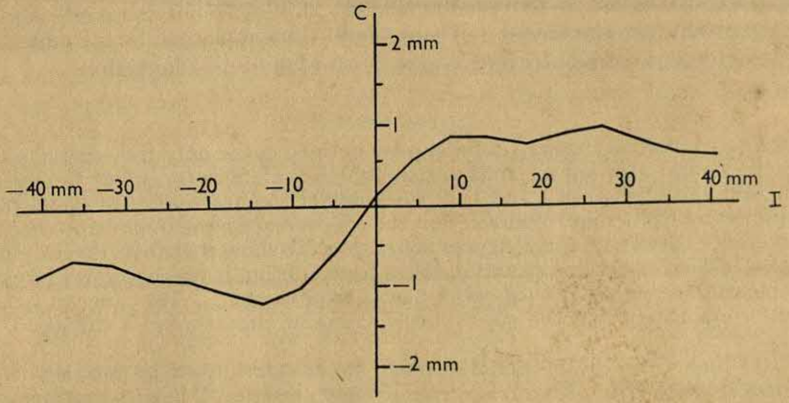
Results

The results of this experiment are plotted in Figure 4, showing the apparent curvature of a straight test line for varying curvature of I. The curve is smoothed as for Experiment 1, and the results for the two subjects are averaged. The after-effect rises rapidly to a maximum as difference between I and T increases, and thereafter the effect falls slowly with increasing difference between I and T. The asymmetry in the curve may be due to the different shapes of the figure for left and right, the difference becoming more obvious with greater curvatures. For I concave right, illustrated by the bracket thus: (, T will be apparently concave left, thus:) and when C matches the apparent curvature of T it will be concave right: it follows that the composite C and T figure will appear to be spool-shaped, thus:) (. For I concave left, the composite C and T figure will appear to be barrel-shaped, thus: ( ).

Experiment III

Although in our Experiments 1 and 11 we use tachistoscopic presentation of curved lines, and Köhler and Wallach used free presentation of the lines in their tilted line experiment, (see their Fig. 61), the results should nevertheless agree. Yet they are in conflict, and no way could be found to reconcile them. The relevant part of their experiment was therefore repeated, keeping as far as possible to the method described in their paper.

FIGURE 4



After-effect on a straight T line for a range of curvatures of inspection figure (Results of Experiment II). With no after-effect C would be zero for every curvature of I; that is to say, the T line would be matched by a straight C line for every I.

The point which Köhler and Wallach were demonstrating by this experiment is that there is no special adaptation of the tilted line to the norm, their thesis being that the interchanging of I and T figures has no effect on the magnitude of the after-effect; in other words, the after-effect of a vertical I on T sloping at 10° is the same in magnitude as that of I sloping at 10° on a vertical T.

In their experiment, Köhler and Wallach used a black I line 3½ in. long, and a T line 3 in. long set between parallel C lines 3 in. long on sides of a 4 in. square hole through which the T line was seen. By tilting the square, the C lines on its sides would be tilted. Thus the apparent tilt of the T line would be compared with the tilt of the C lines.

The subject was shown the I line for 40 sec., during which time he fixated a small cross on the centre of the line. The disc bearing the I line was then removed, exposing the T and C lines; the subject judged whether the T line was tilted to right or left of the C lines. For successive judgements the angle of the T line was altered so that the magnitude of the after-effect could be obtained. The subject had 2 min. rest between inspections and not more than 10 inspections in succession. (There will in general be an interaction between size of angle and measured after-effect when using this method, which as Köhler and Wallach point out is not strictly a measurement of the after-effect on T at the vertical or at the specified angle of 10°, but is rather a measurement of the angle for which the after-effect, plus the angle of T, gives 10° apparent slope. A correction can be made if necessary with the help of measurements at other angles of T; but in general the two measures will increase together so that the difference between them is a second order effect. In this part of their experiment the difference will be even smaller because the angle of 10° at which C is set is near that for maximum after-effect, so that there will be little change of after-effect with change of angle. In any case, the symmetry which Köhler and Wallach were examining would not be affected by an error from this cause).

In our experiment we followed their procedure, but restricted ourselves to two of the conditions used by Köhler and Wallach, that with I at 10°, C at 0°, and that with I at 0°, C at 10°. In both conditions, T was varied over an appropriate range in steps of 1/3°. The vertical is 0°.

Six subjects were used, each being tested in the two conditions. A seventh subject was rejected because his judgements in one of the conditions were several times longer delayed than in the other.

Results

The after-effects in the two conditions for all the subjects are shown in Table I.

Of the six subjects, two showed little difference between the two conditions and the remaining four showed a larger after-effect in condition A (sloping I) than in

TABLE I

FIGURAL AFTER-EFFECT WITH SLOPING AND VERTICAL INSPECTION FIGURES

Subject	1	2	3	4	5	6	*Average*	*Köhler & Wallach Average*
Sloping I 10° ..	1·33	1·08	1·67	1·67	1·56	1·25	1·43	1·21
Vertical I 0° ..	0·88	1·21	1·16	0·75	1·43	0·92	1·06	1·28
Difference ..	0·45	−0·13	0·51	0·92	0·13	0·33	0·37	−0·07

The right-hand column shows results derived from Köhler and Wallach's experiments.

condition B (vertical I). Taking all subjects together, the after-effect for sloping I was 1·35 times that for vertical I, a clear indication that the interchange of I and T lines influences the magnitude of the after-effect. A *t* test shows that the difference between the two conditions is significant at the 5 per cent. level.

The mean after-effect for all subjects in both conditions together is 1·25°, equal, with unexpected accuracy, to the average of 1·25° obtained from Köhler and Wallach's results.

DISCUSSION

From the results of our experiments it can be seen that both adaptation and repulsion work together in producing the figural after-effect, and that neither is sufficient in itself.

Gibson, it will be remembered, noticed adaptation and missed the repulsion. If only adaptation were acting, the result of using a curved T line would be to shift the curve of Figure 4 vertically up or down, so that a range of T lines would produce curves on Figure 3 which pass through the points marked by triangles. That is, each curve would pass through its point of no after-effect as it crosses the line representing a straight I. On the other hand if only repulsion were acting, as claimed by Köhler and Wallach, there would be no after-effect when I and T coincide; the result would be curves with their points of no after-effect on the diagonal line marked by circles which is the line of subjective equality.

The experimental curves of Figure 3 are intermediate between these conditions, showing that the total after-effect has both adaptation and repulsion, either of which may predominate in appropriate conditions. In Nozawa's experiment it is shown that there is a reduction of apparent curvature even when T has greater curvature than I, i.e. a dominance of adaptation; but from Figure 3 we can see that if T has still greater curvature, large when compared with I, then repulsion over-rides adaptation and the apparent curvature of T is increased rather than reduced.

Our results are in agreement, as far as a comparison is possible, with those of Fox, who used a very different set of figures, rectangles for I and a rectangle of four dots as the T figure. Fox found adaptation, repulsion, and the distance paradox.

Our experimental results are in contrast to those of Köhler and Wallach, who found no adaptation to vertical or horizontal but only a repulsion between contours. As no reason could be found why our experiments with curved lines should give qualitatively different results from their experiment with tilted lines, we repeated their experiment as nearly as possible, restricting it, however, to the two conditions of interest here. Our results conflict with those of Köhler and Wallach and do not support them in their assertion that I and T figures can be interchanged without changing the magnitude of the after-effect. We find a difference between the two conditions, where they found none, and this difference is in the direction which would be expected if adaptation as well as repulsion were important in determining the figural after-effect.

If we assume that both adaptation and repulsion are present, how does this affect the theory of the after-effect? The two phenomena appear to be very different from each other, and may sometimes act in opposite directions; yet a hint of a connection appears in that Osgood and Heyer (1952), in a repulsion theory intended to replace the satiation theory of Köhler and Wallach, derive the repulsion from differential adaptation of cells, and in that Gibson regarded adaptation to a norm as analogous to sensory adaptation. It seems reasonable to ask whether physiological adaptation of cells can account for both adaptation and repulsion in the after-effect. In discussing this question we shall adopt a modification of the Osgood and Heyer theory.

Osgood and Heyer make several assumptions which we summarize here:—(*a*) the representation of a contour in the projection cortex is a normal distribution of excitation; (*b*) with nearly steady fixation on a figure the active cells will become differentially adapted; (*c*) the apparent localization of a contour in subjective space will be determined by the location of maximal excitation in area 17. From these assumptions the repulsion after-effect can be derived thus:— The presentation of the I figure would excite a normal distribution of activity in the cortex, and the active cells would adapt to the new level of activity, becoming less excitable. The T figure when presented would arouse activity which overlaps the position of the previous I-activity, and because of the lowered excitability, due to adaptation of cells, the T-activity will be relatively less on the I side. Thus the residual T-activity would have its peak further from the location of I, with an apparent shift of the T line away from the position of I.

Before extending this argument it is necessary to remark on two aspects of the theory which are incidental and will not be carried over into the present work. The first is that Osgood and Heyer suppose that the distribution of excitation depends on small eye-movements across the contour, and is therefore a statistical distribution. For the argument as presented here it is not necessary to invoke eye-movements, it being more consistent to suppose that the distribution of activity is controlled by the differential excitability of the nervous pathways. In any case, eye-movements cannot be a cause of the figural after-effect, for it has been shown that the effect occurs when the image on the retina is stabilized. (P. Nye, private communication, and Krauskopf (1960)). The second incidental is the supposition that the distribution is spatial, on the cortex. Systems of cells with different spatial arrangements in the cortex may nevertheless have the same inter-connections and thus the same behaviour; thus it is a functional distribution of activity and a functional location which we must consider, without excessive regard for position in the cortex. With these reservations, their theory will serve our purpose.

To explain adaptation to a norm we must add to the Osgood and Heyer theory an assumption that adaptation in some cells is very much slower than that in others, taking longer to develop and longer to fade away. If the process were sufficiently slow, excitability would be unevenly distributed as a result of previous activity. In this uneven distribution, a norm would correspond to a position at which activity has been frequent, at which therefore adaptation of cells has marked a position of minimum excitability. If then a T figure gives rise to activity which is near the norm and overlaps it, the activity would be displaced away from the lowered excitability of the cells at the norm; hence the apparent position of T is at first displaced away from the norm (Fig. 5*a*). As the subject continues to inspect T, those cells which are more active become less sensitive and because a proportion of the depression in excitability is due to the new activity, the depression becomes more symmetrical with respect to that new activity and thus deflects it less than before (Fig. 5*b*). This brings the apparent position of T closer to the norm, the physiological adaptation of cells to the level of activity thus producing adaptation to a norm; the same adaptation of cells served as an explanation of repulsion in the original Osgood and Heyer theory.

FIGURE 5

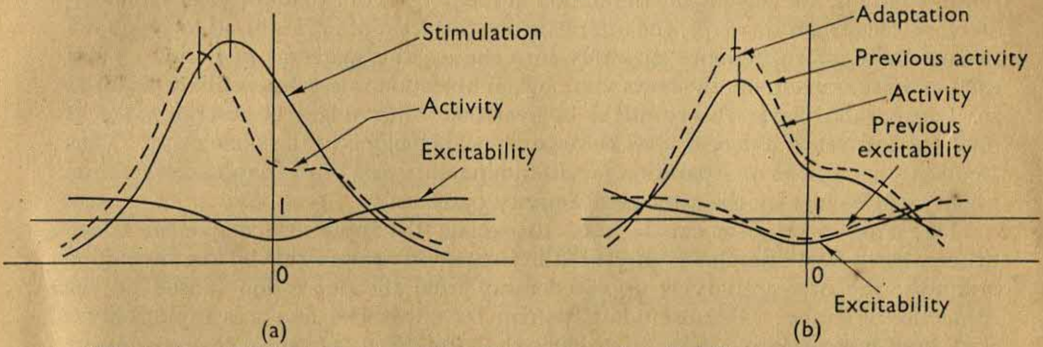


Illustration of Adaptation to a Norm by change of excitability.

(*a*) Initial activity given the stimulation and excitability distributions, showing initial repulsion from the norm.

(*b*) Activity after change of excitability, showing adaptation to the norm.

The curves are calculated as follows:—

It is assumed that there is an arbitrary distribution of excitability of cells, with a dip at the norm because of previous experience; and also that the average excitability of cells is near unity. This arbitrary distribution is sketched in Fig. 5*a*. Then an arbitrary peaked distribution of excitation is assumed to derive from some stimulus, and this is sketched into the figure. The activity at each position can then be calculated as the product of stimulation times excitability; this activity is seen to be displaced away from the norm.

In Figure 5*b* it is supposed that the cells have adapted to the level of activity, the excitability of each cell changing so as to bring its activity closer to the average level of unity. A formula was assumed from which a new distribution of excitability could be calculated and also a new distribution of activity for the same stimulus. The activity has shifted slightly towards the norm, showing an adaptation effect.

A difficulty arises because we suppose that both fast and slow changes of adaptation-level take place, and that both play their part in producing the figural after-effect. Fast changes are necessary because a figural after-effect can be of full magnitude after an inspection as brief as 1 sec. (Ikeda and Obonai, quoted from Sagara and Oyama (1957) and also shown by own own work). Yet some changes must be slow in order to allow a well-established after-effect to survive for minutes or days (Köhler and Wallach), and if the mechanism is to provide a basis for the establishment of norms, then some differentiation of excitability must be even more persistent. We need not suppose, however, that both fast and slow adaptation occur in the same cell. If cells are arranged in a functional hierarchy with rapid adaptation in the peripheral layers and slow adaptation in the deeper layers, the necessary separation is achieved. A stimulus presented to this system would arouse activity which would only briefly be deflected by the peripheral layers of rapidly adapting cells; the adaptation of these cells would reduce their excitability and hence reduce the amount of activity, but adaptation would soon come into correspondence with the activity and any deflection of activity would therefore be brief. In the deeper layers there would be a longer-lasting pattern of excitability which would deflect the activity unless this lay on a norm; the activity would at first be deflected away from the lowered excitability at the norm, then as the excitability of these deeper cells falls by physiological adaptation, the deflection at that level would be lessened and the activity would shift towards the norm. The norm would exist in depth, so that, as superficial norms fade or shift, deeper norms would still persist. Thus when new activity comes into the system its position is determined by the stimulus and by the pre-existence of norms, that is to say by the placing of stimulation in the pattern of excitability; but immediately, as cells begin to adapt, the attendant modification of norms at all levels allows the new activity to fit more smoothly into the existing patterns of norms. (This existence of norms in depth shows also how an objection raised by Deutsch (1956) to the Osgood and Heyer theory might be overcome; lines might be discriminated as different and yet at a deeper level be regarded as belonging to the same class. Thus Deutsch's conditions of separation for discriminability and of overlap for interaction might both be met by the same total activity patterns.)

The same mechanism can be used to explain the repulsion component which arises when a test stimulus is presented in a position near to that of the inspection stimulus; the new activity is displaced away from the depression caused by the inspection stimulus. Because adaptation in the superficial layers is rapid, only a very brief inspection is needed to produce the after-effect, as shown by experiments quoted earlier.

We conclude that the presence of both adaptation and repulsion can be explained by supposing that cells adapt to their level of activity with consequent changes in excitability. With this mechanism adaptation will be dominant where changes are slowest, and repulsion dominant where changes are fastest; a contrast which could readily be tested by experiment.

It has seemed best to present the argument as a development of the Osgood and Heyer theory of the figural after-effect, this being sufficient for the present purpose. One might go beyond this, however, and speculate that a mechanism which modifies the norms of perception, and with them the appearance of things, might well be a fundamental part of the mechanisms of perceptual learning. Leaving aside for the present that aspect of learning which involves fitness for purpose, it could be that adaptation of cells not only modifies the norms but also establishes them, identifying from the flow of stimulation those features which can be grouped as variations from each norm. The succession of stimuli through life would lead to a succession of activity patterns in the brain; those cells which carried most activity would adapt, becoming less excitable, and those which were active more rarely would remain excitable. The distinctive features of the stimulation would be those which do not occur in all stimuli indiscriminately, which therefore would be relatively rare and would be represented by cells which are more excitable. A norm would be the most common appearance of such a distinctive feature, and so would be represented by the less excitable cells within the group. A discussion of this would however lead too far from the present experiments; it is sufficient here to say that the adaptation which Köhler and Wallach denied is nevertheless a component of the figural after-effect and can without difficulty be reconciled with the repulsion component which they discovered.

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SHORT-TERM MEMORY FOR REPEATED AND NON-REPEATED ITEMS

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Digit sequences containing repeated items are retained differently in short-term memory from sequences containing no repeated items. The repeated items are remembered better or worse than items in the corresponding positions of "all-different" sequences depending on the number of times the item is repeated, the number of items repeated, the number of items intervening between the occurences of a repeated item, and the position of the repeated items in relation to the beginning and end of the sequence. In every type of repetition studied, except one, memory for the non-repeated items in sequences with repeated items is better than for the corresponding items of all-different sequences. This is true in some cases despite significant specific interference between the (non-repeated) items following the separated occurences of repeated items. The negative effects in memory for repeated items and the positive effects in memory for non-repeated items are greater when the items are presented at the rate of five per sec. than at one per sec., contrary to the hypothesis that differential rehearsal is responsible for these effects. The results are interpreted as supporting an "associative," as opposed to a "non-associative," theory of short-term memory, as this distinction is defined in the paper.

INTRODUCTION

Ranschburg (1902) studied recognition and immediate recall of six-digit sequences exposed simultaneously for one-third of a sec. He found that sequences with no repeated digits in them (e.g. 702156) were recognized and recalled better than sequences with repeated digits (e.g. 654042), the difference being attributable to poorer recognition and/or recall of the repeated digits.

The method of simultaneous presentation with brief exposure emphasizes the role of recognition processes. In order to investigate *memory* for repeated items it is necessary to assure that both repeated and non-repeated items are recognized. This is best achieved by means of successive presentation.

Kleinknecht (1906) and Turley (1906) used successive presentation and found significant inhibition in the recall of repeated digits only when the identical digits were next to each other and found greater inhibition when the run of two identical digits was in the middle of the sequence than when the run was at the ends. Obonai and Tatsuno (1954) and Tatuno (1961) found *facilitation* for runs of two identical digits at any position in the sequence. Obonai and Tatsuno found inhibition only when the repeated items were separated by several intervening items and then only when the repeated items were not both near the ends of the sequence. In the latter case there was facilitation of memory for the repeated items. Resolution of these contradictory findings is made more difficult by the lack of sufficient description in the above studies of the recall procedure and the method of scoring errors. Furthermore, the absence of any statistical analysis of the size of the effects in relation to the variance permits the conclusion that these findings are chance fluctuations.

The first purpose of the present study is to replicate these studies in order to resolve the contradictory findings and to determine whether the effects are due to differential error rates in ordering the items recalled (as Obonai and Tatsuno suggest) or in recalling items, scored irrespective of order.

The second purpose of the study is to investigate the effects of item repetition on memory for the *non-repeated items* in sequences with repeated items. Tatuno reported serial position curves for sequences with runs of two identical digits at the beginning, middle, and end in which there appeared to be some facilitation of memory for the non-repeated digits compared to digits in comparable positions of sequences with no repeated digits, but there was no indication whether the effect was significant. Tatuno obtained interference in memory for non-repeated items in the other sequence he studied where the first and the fifth items were identical in a sequence of eight items, but again there was no estimate of significance. Also, there were only two different examples of each type of sequence, which seems a little too few to control for other differences between the sequences.

The third purpose of the study is to investigate memory for repeated and non-repeated digits in sequences with runs of three identical digits or two runs of two identical digits at various serial positions.

The fourth purpose of the study is to determine how memory for repeated and non-repeated items is affected by presentation rates that allow more or less time for rehearsal.

METHOD

Experiment 1: *One per sec. rate*

Twenty-six subjects were given sequences of digits of lengths 6, 7, 8, 9, and 10, recorded on tape and presented at the rate of one digit per sec. Subjects had a 20 sec. interval between the end of one sequence and the beginning of the next sequence in which to record, in order, the sequence just heard, leaving blanks to indicate positions for which they could not recall the correct item and did not wish to guess. Subjects were not allowed to write down more items than the sequence contained.

Before the start of the test, subjects were given 21 practice sequences of lengths four to ten with no repeated items. In the test, subjects were given 20 sequences at length 6, then 20 at length 7, . . . , and finally 20 at length 10. The 20 sequences at each length included three that contained no repeated items ("all-different" sequences), and one of each of the following 17 types of sequences containing repeated items:

(ii$_B$)	1 digit repeated in a length-2 run at beginning of the sequence	(773519)
(ii$_M$)	1 digit repeated in a length-2 run at middle of the sequence	(258830)
(ii$_E$)	1 digit repeated in a length-2 run at end of the sequence	(904211)
(iji$_B$)	1 digit repeated, separated by a digit at beginning of the sequence	(373068)
(iji$_M$)	1 digit repeated, separated by a digit at middle of the sequence	(154596)
(iji$_E$)	1 digit repeated, separated by a digit at end of the sequence	(043676)
(ijki$_B$)	1 digit repeated, separated by 2 digits at beginning of the sequence	(518502)
(ijki$_M$)	1 digit repeated, separated by 2 digits at middle of the sequence	(903702)
(ijki$_E$)	1 digit repeated, separated by 2 digits at end of the sequence	(239749)
(i—i$_{BE}$)	1 digit repeated, separated by the rest of the sequence	(410894)
(iijj$_B$)	2 digits repeated in length-2 runs at beginning of the sequence	(995536)
(ii—jj$_{BE}$)	2 digits repeated in length-2 runs at the 2 ends of the sequence	(881377)
(ijij$_B$)	2 digits repeated in mixed pairs at beginning of the sequence	(616180)
(ij—ij$_{BE}$)	2 digits repeated in mixed pairs at the 2 ends of the sequence	(760476)
(iii$_B$)	1 digit repeated in a length-3 run at beginning of the sequence	(333240)
(iii$_M$)	1 digit repeated in a length-3 run at middle of the sequence	(400065)
(iii$_E$)	1 digit repeated in a length-3 run at end of the sequence	(294777)

Subject to the constraints specified above, sequences were randomly selected. Also, the sequences were randomly ordered within each block of 20 sequences, subject to the constraint that one of the three all-different sequences appeared in each third of each set of 20. Then a totally new set of 20 sequences at each length was randomly selected, subject to the constraints imposed by the different types of sequences. The second set of sequences was arranged in the reverse order of the first set of sequences at each length to control for any practice effects remaining after the practice series. The first set was given to 13 subjects and the second set to 13 subjects. Subjects were M.I.T. undergraduates recruited from an introductory psychology course.

Experiment 2: *Five per sec. rate*

The procedure was similar to experiment one except that the rate of presentation was five digits per sec. Subjects were given 11 sequences at length 6, then 11 at length 7, . . ., then 11 at length 10, making a total of 55 sequences in each set of sequences. There were three different sets of 55 sequences given to each subject, one set after another, making a total of 165 sequences. The 11 sequences at each length in each set consisted of the all-different sequences, ii_B, iji_B, iji_M, iii_B, iii_E, and $iijj_B$ from experiment one plus the following new types:

(ii_{34})	One run of two identical items in positions 3 and 4 of the sequence	(017752)
(ii_{56})	One run of two identical items in positions 5 and 6 of the sequence	(439600)
(iii_{345})	One run of three identical items in positions 3, 4, and 5 in the sequence	(820004)
($iijjkk_B$)	Three runs of two identical digits at the beginning of the sequence	(774466)

Subject to the constraints specified above, sequences were randomly selected and randomly ordered within each block of 11. Summing over all five lengths, there were 15 different sequences for each of the above 11 types. Subjects were 19 M.I.T. undergraduates from an introductory psychology course.

Results

Experiment 1

The data were analyzed for ordered recall and free recall of individual items and entire sequences. A subject's report of a *sequence* is correct by an *ordered recall* criterion if and only if all items are recalled in the correct order. A subject's report of a *sequence* is correct by a *free recall* criterion if and only if all the items are recalled correctly, irrespective of order. A subject's report of each *item* in a sequence is correct by an *ordered recall* criterion if and only if the correct item is recalled in the correct position. A subject's report of each *item* in a sequence is correct by a *free recall* criterion if and only if it appears anywhere in his report of the sequence in question.

At the one per sec. rate, the 50 per cent. point in ordered recall of entire sequences for these subjects was between length 9 and length 10. There was a very slight practice effect in each block of 20 test sequences as measured by *ordered recall* of items and sequences for the all-different sequences in the first, second, and final third of each block of 20. There was no practice effect in *free recall* of items and sequences.

The Spearman rank order correlation between sequence ordered recall and item ordered recall for the 18 types of sequences was 0·90. The correlation between sequence free recall and item free recall was 0·94. The correlation between sequence ordered and sequence free recall was 0·88, but the correlation between item ordered and item free recall was only 0·76. Since item recall correlates so highly with sequence recall and since analysis by item recall permits decomposition of a sequence into repeated items and non-repeated items, the rest of the paper will consider only item recall statistics.

Table I presents the per cent. error in ordered and free item recall for each sequence type at the one per sec. rate. χ^2 tests were used to provide a rough measure of the significance of differences between the all-different sequence and other sequence types. Since all subjects and all trials for the same subject are pooled, the χ^2 test must be interpreted accordingly.

Sequences with certain types of item repetition are remembered significantly better than all-different sequences, but sequences with other kinds of item repetition are remembered no better and in some cases significantly worse than all-different sequences. A run of three identical items at the beginning, middle, and end of a sequence facilitates short-term memory, but the effect is greater at the beginning than at the middle or at the end. A run of two identical items at the beginning or end facilitates short-term memory, but a run of two in the middle does not. A run of two is always less effective than a run of three at any position. An iji pattern at the beginning of a sequence facilitates short-term memory for the sequence, but iji patterns at other positions and ijki patterns at any position do not. Sequences with the same item at the beginning and end (i—i$_{BE}$) are remembered significantly *worse* than all-different sequences. Sequences beginning with iijj and ijij patterns are remembered better than all-different sequences or sequences beginning with ii patterns. Sequences with the repeated items divided between the beginning and the end (ii—jj$_{BE}$ and ij—ij$_{BE}$) are remembered no better than all-different sequences. In fact, ij—ij$_{BE}$ is almost significantly worse than the all-different sequence at the 0·05 level.

TABLE I

AVERAGE ERROR RATE FOR ITEMS FROM DIFFERENT SEQUENCE TYPES

(One per sec. rate)

	Ordered Recall			*Free Recall*		
Type	*Per cent*	*Sign*	*p*	*Per cent*	*Sign*	*p*
A.D.	11·86			4·46		
ii$_B$	8·27	+	**	2·88	+	*
ii$_M$	11·15	+		4·90	—	
ii$_E$	8·08	+	**	3·85	+	
iji$_B$	8·85	+	**	3·46	+	
iji$_M$	11·15	+		4·33	+	
iji$_E$	11·06	+		5·10	—	
ijki$_B$	12·50	—		4·71	—	
ijki$_M$	11·54	+		5·00	—	
ijki$_E$	12·02	—		3·75	+	
i—i$_{BE}$	14·71	—	*	5·96	—	*
iijj$_B$	6·63	+	***	2·88	+	*
ii—jj$_{BE}$	11·06	+		5·38	—	
ijij$_B$	7·21	+	***	1·73	+	*
ij—ij$_{BE}$	14·13	—		5·00	—	
iii$_B$	3·65	+	***	0·96	+	*
iii$_M$	5·38	+	***	3·08	+	
iii$_E$	9·04	+	*	3·75	+	

A.D. = All-different sequence.
Sign = Algebraic sign of the difference in error rate from A.D. sequences.
p = Probability of difference in error rate from A.D. sequences.
Two-Tailed Chi Square Test: * = 0·05, ** = 0·01, *** = 0·001.

At this point we have established the effects of item repetition on the error rate for items, averaged over all items (repeated and non-repeated) in the sequence. In each comparison of sequences having repeated items with the all-different sequence, two further questions must be asked: (1) What is the error rate for only the repeated items as compared to the error rate for items in the identical positions in the all-different sequence? (2) What is the error rate for the non-repeated items in sequences with repeated items as compared to the error rate for the (non-repeated) items in identical positions in the all-different sequences?

To answer these questions it is necessary to compute the ordered and free recall error rates separately for repeated and non-repeated items. Prior studies of short-term memory for repeated items have reported only ordered recall statistics. Unfortunately, ordered recall scoring contains certain statistical artefacts favouring sequences with repeated items. One artefactual advantage of sequences with repeated items is that there can be no transposition error between repeated items. Another advantage is that a run of identical items that is displaced in recall by one from its correct position, still has n-l of its items in correct positions, whereas a sequence of non-identical items displaced by one in recall has none of its items in correct positions. Therefore, the ordered recall data in Table I should be interpreted with caution.

Tables II and III report the error rates *separately* for repeated and non-repeated items in comparison with items in the identical positions in the all-different sequences. Free recall error rates are computed the same as before, but "position recall" error rates are a new way of scoring ordered recall that is free of artefactual advantages for repeated items and statistically independent of the free recall data. Independence from the free recall data is achieved by scoring for correct position in the sequence *only* the items that are recalled correctly by a free recall criterion. For position recall comparisons with *repeated* items, comparable items from the all-different sequences are scored as an entire *set*, all of whose members must be in the correct *set of positions* in the sequence in order to be correct, but within the set itself transposition errors do not count as errors. Naturally the repeated items are scored the same way for position recall. For example, by this new position recall scoring procedure the repeated items from a sequence beginning with a run of three 4's (444 . . .) are

TABLE II

ERROR RATE FOR REPEATED ITEMS COMPARED TO IDENTICAL POSITIONS IN A.D. SEQUENCES

(One per sec. rate)

	Free Recall (*Per cent.*)				*Position Recall* (*Per cent.*)			
Type	*R.I.*	*A.D.*	*Sign*	*p*	*R.I.*	*A.D.*	*Sign*	*p*
ii_{B} ..	1·15	4·23	+	*	1·56	6·37	+	
ii_{M} ..	6·15	3·59	—		6·03	11·54	+	
ii_{E} ..	1·54	1·15	—		0·00	5·26	+	*
iji_{B} ..	4·23	4·36	+		3·33	10·16	+	*
iji_{M} ..	7·31	4·62	—		8·18	12·04	+	
iji_{E} ..	6·15	2·82	—	*	5·26	8·72	+	
$ijki_{B}$ ..	5·77	3·85	—		8·55	10·74	+	
$ijki_{M}$ ..	9·62	5·64	—	*	8·33	12·96	+	
$ijki_{E}$ ..	4·62	2·44	—		7·89	9·97	+	
i—i_{BE} ..	4·62	1·28	—	**	2·54	1·32	—	
$iijj_{B}$ ..	4·23	5·06	+		1·72	7·62	+	*
ii—jj_{BE} ..	5·38	2·69	—	**	4·76	9·69	+	
$ijij_{B}$ ..	1·35	5·06	+	***	4·03	8·85	+	
ij—ij_{BE} ..	5·00	2·69	—	*	8·93	9·69	+	
iii_{B} ..	1·54	4·96	+	**	0·00	7·14	+	**
iii_{M} ..	3·33	4·36	+		3·39	9·17	+	
iii_{E} ..	2·05	2·56	+		0·00	7·71	+	**

R.I. = Sequences with repeated items.

Position Recall is independent of free recall and free of artefacts.

Sign = Algebraic sign of difference in error rate between repeated items and comparable items from A.D. sequences.

p = Probability of difference in error rate between repeated items and comparable items from A.D. sequences. (Two-Tailed Chi Square Test: * = 0·05, ** = 0·01, *** = 0·001.)

counted as either correct or incorrect depending on whether or not all three 4's are recalled in the first three positions. An all-different sequence beginning with 413, scored in a comparable way, is also counted as either correct or incorrect depending on whether or not 4 and 1 and 3 are all recalled in the first three positions. Notice that is is not necessary that the order of recall be 413 . . . ; any transposition of these three numbers in the first three positions is equally correct. Position recall comparisons of *non-repeated* items from sequences with and without repeated items do not require scoring as a set to avoid artefacts, and therefore in this case an individual item is scored as correct or incorrect depending on whether it was recalled in the correct or incorrect position in the sequence.

TABLE III

ERROR RATE FOR NON-REPEATED ITEMS IN SEQUENCES WITH REPEATED ITEMS AND IN A.D. SEQUENCES

(One per sec. rate)

Type	*Free Recall (Per cent.)*				*Position Recall (Per cent.)*			
	R.I.	*A.D.*	*Sign*	*p*	*R.I.*	*A.D.*	*Sign*	*p*
ii_B	3·46	4·53	+		6·77	9·00	+	
ii_M	4·49	4·74	+		6·71	6·55	—	
ii_E	4·62	5·56	+		5·91	9·46	+	***
iji_B	3·21	4·49	+		6·49	8·50	+	
iji_M	3·33	4·40	+		7·03	6·79	—	
iji_E	4·74	5·00	+		7·40	8·86	+	
$ijki_B$	4·36	4·66	+		9·38	8·38	—	
$ijki_M$	3·46	4·06	+		7·04	7·22	+	
$ijki_E$	3·46	5·13	+		9·96	8·60	—	
$i\text{—}i_{BE}$	6·41	5·51	—		12·05	10·22	—	
$iijj_B$	1·53	3·85	+	*	6·25	8·27	+	
$ii\text{—}jj_{BE}$	5·38	6·22	+		10·77	12·24	+	
$ijij_B$	2·12	3·85	+	*	8·64	8·27	—	
$ij\text{—}ij_{BE}$	5·00	6·22	+		16·40	12·24	—	*
iii_B	0·62	4·15	+	***	4·33	8·77	+	***
iii_M	2·92	4·51	+		3·01	5·80	+	**
$iiii_E$	4·77	5·59	+		8·89	9·56	+	

Position Recall is independent of free recall.

Sign = Algebraic sign of difference in error rate between non-repeated items in sequences with repeated items and comparable items from A.D. sequences.

p = Probability of difference in error rate between non-repeated items in sequences with repeated items and comparable items from A.D. sequences. (Two-Tailed Chi Square Test: * = 0·05, ** = 0·01, *** = 0·001.)

Perhaps the most striking fact is that free recall memory is very much better for repeated items in certain sequences (ii_B, $ijij_B$, and iii_B) than comparable items in the all-different sequences and very much worse for repeated items in other sequences (iji_E, $ijki_M$, $i\text{—}i_{BE}$, $ii\text{—}jj_{BE}$, and $ij\text{—}ij_{BE}$) than comparable items in the all-different sequences. It appears that memory for the repeated items is better for runs of three than for runs of two, better when fewer items separate the occurrence of the repeated item(s) and better for repetitions occurring near the beginning of the sequence. It is reasonable to conjecture that these conditions favour recoding of a run or simple repetition-pattern into one chunk, or at least fewer chunks than the number of items in the run or repetition-pattern. Of even greater interest is the finding that when conditions are (presumed to be) unfavourable for recognition of the repetition and recoding into fewer chunks, repeated items are remembered worse than comparable non-repeated items.

In 16 out of 17 comparisons, position recall is better for repeated items than for comparable items in the all-different sequences. Since position recall is computed only for those items recalled correctly by a free recall criterion, this further supports the hypothesis that when the repetition is recognized and recoded by the subject the repeated items are remembered better than the comparable non-repeated items.

In 16 out of 17 cases, free recall memory for comparable non-repeated items is better in sequences with repeated items than in sequences without repeated items. It appears that repeated items produce less interference with the rest of the items in the sequence than comparable non-repeated items. This is true regardless of whether the repeated items are remembered better or worse than comparable non-repeated items from the all-different sequence. The only consistent exception to this generalization is $\mathrm{i—i_{BE}}$.

Position recall is superior for non-repeated items from sequences with repeated items in only 10 comparisons out of 17; three comparisons are significant in one direcdirection, and one in the opposite direction. No simple explanation accounts for the pattern of relationships. The reduced interference from repeated items on the recall of non-repeated items is measured primarily in superior recall of items, not in superior recall of position.

For reasons that will be explained in the discussion, let us consider the kinds of intrusion errors made in place of the item *following* (correctly recalled) repeated items in the following sequences: $\mathrm{iji_B}$, $\mathrm{iji_M}$, $\mathrm{ijki_B}$, and $\mathrm{ijki_M}$. We are concerned with how often this intrusion error is the item that followed the *other* occurence of the repeated item in the *presented* sequence. For example, in the sequence ijikl . . . we are interested in the frequency of putting k in place of j in the second position and the frequency of putting j in place of k in the fourth position, compared to the frequency of all other intrusion errors at these positions in these sequences. Call this type of intrusion an "associative intrusion." Averaging over all four sequence types mentioned above, associative intrusions occurred 16 times out of a total of 50 intrusion errors, or 32 per cent. of all intrusion errors in these positions. Scoring intrusion errors into the same two categories in the all-different sequence, we obtain 27 intrusions comparable to the associative intrusions, out of a total of 143 intrusions of all types, or 19 per cent. of all intrusions in these positions. Scoring the all-different sequence in the same way provides an estimate of how frequently the items in the positions with which we are concerned would be substituted for each other, even when they did not follow identical items. The difference between 32 per cent. and 19 per cent. just misses significance at the 0·05 level ($\chi^2 = 3·68$), providing some support for the hypothesis that items following repeated items are more likely to be substituted for each other in recall than items following comparable non-repeated items.

Experiment 2

At the five per sec. rate, the 50 per cent. point in ordered recall of entire sequences was almost exactly at length 8. Error rates in ordered and free item recall of each sequence type are presented in Table IV. All 10 types of sequences with repeated items are remembered better than the all-different sequences by ordered recall scoring. By free recall scoring, $\mathrm{ii_B}$, $\mathrm{iji_B}$, $\mathrm{iii_B}$, and $\mathrm{iii_E}$ are remembered significantly better than the all-different sequence, while $\mathrm{iii_{345}}$ is remembered significantly worse.

Tables V and VI report the free recall and position recall error rates for repeated and non-repeated items in comparison with items in the identical positions in the all different sequences.

Free recall memory for repeated items is significantly better than for comparable items in the all-different sequence in only one case, ii$_B$. Free recall memory for repeated items is significantly worse in five cases: ii$_{34}$, ii$_{56}$, iii$_{345}$, iijjkk$_B$, and iji$_M$. As at the one per sec. rate, memory for items is also much better for some sequences and much worse for other sequences than memory for items at identical positions in the all-different sequences. At both rates, repeated items in the middle show the greatest decrement in recall. At both rates, it appears that repeated items are remembered worse when they are separated by other items than when they are together in a run.

TABLE IV

AVERAGE ERROR RATE FOR ITEMS FROM DIFFERENT SEQUENCE TYPES

(Five per sec. rate)

Type	*Ordered Recall*			*Free Recall*		
	Per cent.	*Sign*	*p*	*Per cent.*	*Sign*	*p*
A.D.	24·28			7·83		
ii$_B$	19·44	+	***	4·90	+	***
ii$_{34}$	20·74	+	**	9·22	—	
ii$_{56}$	23·00	+		8·62	—	
iji$_B$	18·74	+	***	5·10	+	***
iji$_M$	24·20	+		9·18	—	
iii$_B$	21·29	+	*	5·58	+	**
iii$_{345}$	22·52	+		11·43	—	***
iii$_E$	15·71	+	***	5·12	+	***
iijj$_B$	20·18	+	**	6·91	+	
iijjkk$_B$	16·31	+	***	8·43	—	

Sign = Algebraic sign of the difference in error rate from A.D. sequences.
p = Probability of difference in error rate from A.D. sequences.
(Two-Tailed Chi Square Test: * = 0·05, ** = 0·01, *** = 0·001.)

However, there are some interesting differences between the one per sec. rate and the five per sec. rate. There seems to be a general tendency for repeated items to be remembered worse at the faster rate of presentation than at the slower rate. Runs of three identical items, particularly, are remembered much worse at the faster rate of presentation. At the one per sec. rate, runs of three identical items at the beginning are remembered better than runs of three at the end, but, at the five per sec. rate, just the reverse is true. At the one per sec. rate, runs of three are remembered better than runs of two, but at the five per sec. rate, the reverse is true. The decrement in recall of runs of three identical items at the beginning develops gradually with increasing length of the sequence, and is accounted for completely by a tendency to omit the third item of the run. This tendency increases with sequence length, as the number of items following the run of three increases. At length-6, runs of three identical items at the beginning are remembered better than the three items at the beginning of all-different sequences, but by lengths 9 and 10 the identical items are remembered worse.

Recall of the correct position, for items correctly recalled at some position, is better for repeated items than for the comparable items in the all-different sequences in 10 cases out of ten. In every sequence type except iijjkk$_B$ and iji$_M$, this effect is significant. In general, repeated items are less likely to be recalled at all, but those that are recalled are more likely to be recalled in the correct position, whether the rate is five per sec. or one per sec.

TABLE V

ERROR RATE FOR REPEATED ITEMS COMPARED TO IDENTICAL POSITIONS IN A.D. SEQUENCES

(Five per sec. rate)

	Free Recall (Per cent.)				Position Recall (Per cent.)			
Type	*R.I.*	*A.D.*	*Sign*	*p*	*R.I.*	*A.D.*	*Sign*	*p*
ii$_{B}$ ..	0·34	3·14	+	***	0·00	9·58	+	***
ii$_{34}$ ..	14·23	7·25	—	***	4·81	22·57	+	***
ii$_{56}$ ..	20·80	10·59	—	***	10·18	26·57	+	***
iji$_{B}$ ..	2·16	3·53	+		4·51	14·29	+	***
iji$_{M}$ ..	19·70	9·61	—	***	23·75	47·26	+	
iii$_{B}$ ..	4·62	4·31	—		6·22	13·22	+	*
iii$_{345}$ ..	19·08	7·84	—	***	10·95	23·19	+	**
iii$_{E}$ ..	4·14	6·01	+		0·83	17·54	+	***
iijj$_{B}$ ..	8·67	5·20	—		9·55	19·16	+	*
iijjkk$_{B}$ ..	9·55	6·99	—	*	7·14	11·05	+	

Position Recall is independent of free recall and free of artefacts.

Sign = Algebraic sign of difference in error rate between repeated items and comparable items from A.D. sequences.

p = Probability of difference in error rate between repeated items and comparable items from A.D. sequences.

(Two-Tailed Chi Square Test: * = 0·05, ** = 0·01, *** = 0·001.)

TABLE VI

ERROR RATE FOR NON-REPEATED ITEMS IN SEQUENCES WITH REPEATED ITEMS AND IN A.D. SEQUENCES

(Five per sec. rate)

	Free Recall (Per cent.)				Position Recall (Per cent.)			
Type	*R.I.*	*A.D.*	*Sign*	*p*	*R.I.*	*A.D.*	*Sign*	*p*
ii$_{B}$ ..	6·48	9·42	+	**	17·29	18·40	+	
ii$_{34}$ ..	7·52	8·02	+		12·59	14·70	+	
ii$_{56}$ ..	4·50	6·90	+	**	14·42	12·69	—	
iji$_{B}$ ..	6·12	9·28	+	**	15·70	17·64	+	
iji$_{M}$ ..	5·31	7·23	+		14·13	13·27	—	
iii$_{B}$ ..	6·16	9·98	+	***	20·59	19·43	—	
iii$_{345}$ ..	6·89	7·82	+		14·20	13·03	—	
iii$_{E}$ ..	5·71	8·94	+	**	15·10	16·16	+	
iijj$_{B}$ ..	5·12	10·52	+	***	20·58	19·40	—	
iijjkk$_{B}$ ..	4·94	10·45	+	**	17·76	16·60	—	

Position Recall is independent of free recall

Sign = Algebraic sign of difference in error rate between non-repeated items in sequences with repeated items and comparable items from A.D. sequences.

p = Probability of difference in error rate between non-repeated items in sequences with repeated items and comparable items from A.D. sequences. (Two-Tailed Chi Square Test * = 0·05, ** = 0·01, *** = 0·001.)

At the five per sec. rate, as at the one per sec. rate, non-repeated items in sequences with repeated items are remembered uniformly better than the comparable (non-repeated) items from the all-different sequence; by a free recall criterion. The effect seems to be even stronger at the five per sec. rate than at the one per sec. rate.

Recall of the correct position, for items correctly recalled at some position, is equally good for non-repeated items in sequences with and without repeated items.

Associative intrusion errors account for 36 out of 94 intrusion errors in the positions following the repeated items in iji_B and iji_M, or 38 per cent. The comparable figures for the all-different sequence are 18 out of 113, or 16 per cent. This difference is significant at the 0·001 level ($\chi^2 = 13{\cdot}32$).

DISCUSSION

Let us consider two basic types of memory systems by which a human being might recall an ordered list of items: an associative memory and a non-associative memory.

We shall define an associative memory to be a memory system satisfying two properties: (1) The internal representative of any *item* (item-rep) is a class of firing patterns always in the *same* set of neurons. (2) The internal representative of the *order* in which the items are presented is a facilitation of the connections between internal-reps. This facilitation of the tendency for one internal-rep to activate another internal-rep depends on the degree of contiguity of activation of the two internal-reps in the past and the time or number of items since last pairing. So long as there is no way to prevent rehearsal, degree of contiguity of the *presented items* measured in time is probably vastly less important than degree of contiguity of the presented items measured in number of intervening items.

There are many different associative memory systems for the ordered recall of an ordered list, all based on the above two properties, but differing in their assumptions concerning what internal-reps are activated during presentation of the list. All associative memory systems must assume that the presented items activate item-reps in the memory, but in addition there may be internal-reps of such ordering concepts as "beginning of the list," "next to the beginning," "middle of the list," "next to the end," and "end of the list," that are activated while the list is presented and therefore become associated to certain items and not to others, or more strongly associated to certain items than to others. Furthermore, there may be internal-reps of certain general context cues (pure free recall cues) that become associated weakly, but more or less equally, to all items in the list. These cues facilitate free recall of items, but provide no order information.

We shall define a non-associative memory to be a memory system satisfying two properties: (1) The internal-rep of an item is a class of firing patterns in *any* sufficiently large set of neurons. (2) The internal-rep of the order in which the items are presented is a *fixed connection* between sets of neurons, each of which is large enough to store the firing pattern that is the internal-rep of an item. When a list of items is presented, the firing pattern representing the first item is impressed on the set of neurons that is the first memory cell, then the firing pattern representing the second item is impressed on the second memory cell, etc. In recall, the system starts at the first memory cell and reads its firing pattern, then goes to the second memory cell and reads its firing pattern, etc. It should be mentioned that the trace that is stored in a memory cell need not be active—that is, it may be a change in the synapses that lasts until read-out, not a reverberating firing pattern. In the passive-trace version of the non-associative memory theory, read-out might consist of putting some diffuse input into a memory cell and reading the firing pattern that develops as a result of the passive trace.

The large amount of specific interference between the two different items following the separated occurrences of a repeated item in a list is compelling support for an associative theory of short-term memory. In an associative memory, the internal-rep of the repeated item should be associated to the internal-reps of both succeeding items, and this ought to produce an above chance frequency of associative intrusion errors. In a non-associative memory there is no natural reason for this interference.

The second finding that supports an associative theory of short-term memory is the decrement in the free recall of repeated items in all cases except those most favourable to the recognition and recoding of the repetition. For a non-associative memory, in the absence of recognition and recoding of repeated items, memory for the repeated items should be the same as for comparable non-repeated items. However, for an associative memory there is only one internal-rep of an item, and if both occurrences of the item are not correctly recalled in order, free recall cues will provide only the information that the item was presented, not that it was presented twice.

The fact that certain patterns of repeated items are remembered better than items in the corresponding positions of all-different sequences is evidence for a recoding mechanism in short-term memory. This means that three successive activations of an item-rep activates the internal-rep of "run-of-three—s" which is parameterized at the — by the item that was repeated. This eliciting of new concepts by association to certain patterns of input stimulation is part of the essence of an associative memory. With a non-associative theory of short-term memory, this eliciting of new concepts and recoding must be done by another system separate from the non-associative short-term memory system. This may be how it is done, but such a combination of systems is certainly a less elegant, less parsimonious theory.

When repeated items are recoded into a single internal-rep, such as "run-of-three —s," and thus remembered better, the fact that short-term memory for the non-repeated items is also better can be easily predicted by both associative and non-associative theories. Recoding effectively shortens the list in terms of the number of internal-reps that are needed to encode the list.

When repeated items are not recoded and are then remembered worse than non-repeated items, the facilitation of memory for the non-repeated items is very hard to explain with a non-associative memory theory. With an associative memory in which the interference between items is a function of the number of *different item-reps* activated, this result is very easily explained because lists with one repeated item activate one less different item-rep and thus generate less interference.

Assuming that human short-term memory is associative, what further information about the system is provided by this experiment?

The fact that memory for the items following the separated occurrences of repeated items is not disasterously impaired is evidence for the existence of some kind of serial order cues (such as: beginning, next to beginning, middle, next to end, end) in addition to prior-item cues for the ordered recall of ordered lists.

The fact that free recall of items and ordered recall of items are not perfectly correlated is evidence for a set of general context (free recall) cues associated to all items, or some other mechanism for determining if an internal-rep has been activated recently.

The findings of these experiments have been interpreted as evidence for the associative nature of short-term memory. However, an alternative explanation for the above findings might be advanced in terms of a cognitive strategy of rehearsal and recoding that would predict the same findings and would invalidate these findings as evidence for the associative nature of short-term memory.

Another line of attack on the interpretation that short-term memory is associative is to limit the generality of the interpretation by questioning, "How short is short?" A 10-item sequence presented at one item per sec. takes 9 sec. to present. Is memory for 10 items presented in 9 sec. the shortest of short-term memories or is there a totally different type of memory for 10 items presented in a much shorter period of time?

The second experiment was performed to provide some evidence on both of these questions. Any strategy explanation of the one per sec. findings should predict that the findings would be less significant when the subject had less time to implement whatever strategy he was assumed to be employing. However, the three findings that are most compelling in support of an associative theory of short-term memory, namely, (1) the negative effects in free recall memory for repeated items, (2) the positive effects in free recall memory for non-repeated items in sequences with repeated items, and (3) the above chance incidence of associative intrusions, are all more significant at the faster rate of presentation than at the slower rate of presentation. Strategies of rehearsal and recoding tend to diminish these findings, not enhance them.

Selective attention to items *different* from those items previously heard might account for part of the decrement in recall of repeated items. By this hypothesis subjects might fail to *hear* repeated items more often than non-repeated items. This hypothesis is almost certainly false at the one per sec. rate, but at the five per sec. rate it is certainly possible. At the five per sec. rate the third item of a run of three identical items at the beginning tends to be omitted, and at first glance, this fact seems to argue that perhaps subjects do not hear all three items. However, runs of three identical items at the beginning are remembered even better than comparable non-identical items in shorter length sequences. Only when the sequence gets longer do they tend to omit the third item of a run of three identical items at the beginning. This argues that the locus of the effect is in memory, not in attention. The tentative explanation offered here is that, at the faster speed, runs of three at the beginning do not have as much time to be recoded as "run-of-three —'s" and are therefore less often recoded than at the slower speed. When repeated items are not recoded into a new concept, they are remembered worse than non-repeated items according to the hypothesis that short-term memory is associative.

The results of the second experiment in conjunction with the first experiment make it appear unlikely that strategy explanations (such as rehearsal, recoding, or selective attention) are responsible for the findings in short-term memory for repeated and non-repeated items. If strategy explanations are invalid, the results of these two experiments argue strongly that short-term memory is associative rather than non-associative, even short-term memory as short-term as 10 items presented in 2 sec.

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OVERTRAINING, TRANSFER TO PROPRIOCEPTIVE CONTROL AND POSITION REVERSAL

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If rats are overtrained on a visual discrimination they generally learn the reversal of the discrimination faster than non-overtrained rats. In position discriminations, however, this effect does not generally hold—indeed several investigators have found overtraining to retard position reversal. One of the important differences between the two types of problem seems to be the presence of irrelevant cues in visual discriminations, and their absence in position discriminations. It is suggested that a second important feature of position discriminations is that overtraining usually causes control of the maze running habit to be transferred from external to proprioceptive stimuli, and that successful reversal cannot normally occur until external control is re-established. In two experiments a study by Krechevsky and Honzik (1932) is repeated with certain modifications, with results that support this hypothesis; and a third experiment provides direct evidence of transfer to proprioceptive control in a T-maze. It is shown that this analysis will explain the apparently conflicting results of all recent position reversal experiments.

INTRODUCTION

Many recent studies have shown that the more training rats are given on a simultaneous visual discrimination, the faster they learn the reversal of the discrimination (Reid, 1953; Pubols, 1956; Capaldi and Stevenson, 1957; North and Clayton, 1959; Brookshire, Warren and Ball, 1961; D'Amato and Jagoda, 1961; Mackintosh, 1962, 1963*a*). In only one experiment has this overtraining reversal effect failed to appear (Erlebacher, 1963). The effect of overtraining on the reversal of a position discrimination, however, is much less clear cut. In three experiments overtraining has significantly facilitated reversal (Pubols, 1956; Bruner, Mandler, O'Dowd and Wallach, 1958; Capaldi, 1963), but in nine others it has either had no effect or actually retarded reversal (Galanter and Bush, 1959; D'Amato and Jagoda, 1962—a total of four experiments; Hill, Spear and Clayton, 1962—a total of three experiments; Hill and Spear, 1963).

Evidence has been presented elsewhere (Mackintosh, 1962, 1963*a* and *b*, 1964) to show that the overtraining reversal effect can be understood in terms of a model of discrimination learning suggested by Sutherland (1959). The basis of this explanation is the assumption that overtraining causes animals to respond in terms of the relevant cue throughout reversal (by preventing extinction of the relevant analyser), whereas non-overtrained animals switch out the relevant analyser upon reversal, respond to other, irrelevant cues, and hence are delayed. On this account, the presence of irrelevant cues in the discrimination situation is of crucial importance in producing the overtraining reversal effect: if there are no irrelevant cues, non-overtrained animals will not switch in irrelevant analysers, and are therefore unlikely to be significantly delayed. Evidence from both rats (Mackintosh 1963*a*) and octopuses (Mackintosh and Mackintosh, 1963) supports this suggestion. Now in simultaneous visual discriminations, the position of the positive and negative stimuli varies from trial to trial, and thus acts as an irrelevant cue; but in a simple T-maze, there are no irrelevant cues deliberately manipulated by the experimenter. On this explanation, therefore, overtraining would not be expected to facilitate position reversal, unless irrelevant cues were deliberately introduced, and the presence of irrelevant cues in one of the experiments where overtraining did facilitate reversal (Pubols, 1956) provides good support for the theory.

Nevertheless this analysis fails to explain the results of several other experiments. On the one hand, Capaldi (1963) found an overtraining reversal effect when rats were trained on an elevated T-maze (all other experiments have been on enclosed mazes); Bruner *et al.* (1958) found the effect in a 4-unit maze; and Ison and Birch (1961) obtained it in an enclosed T-maze when training and overtraining involved merely *placing* the animals in positive and negative goal-boxes a certain number of trials (i.e. not letting them run through the maze until reversal learning began). There are thus three clear-cut demonstrations of an overtraining reversal effect in the absence of irrelevant cues. On the other hand, in several other studies, overtraining has actually retarded position reversal, and although the above analysis predicts that overtraining will not facilitate reversal in a simple T-maze, it cannot explain why overtraining should sometimes retard reversal (e.g. in Hill *et al.*, 1962, 1963, and Krechevsky and Honzik, 1932).

Although the original theory may be correct as far as it goes, it is clear that some supplementary hypothesis is needed. One possibility is suggested by an examination of Krechevsky and Honzik's experiment. These investigators trained rats to select the shorter of two paths to food, and then reversed the lengths of the two paths, so that the rats now had to select the previously longer path in order to get food with the shortest possible delay. The more training given on the initial problem, the longer it took to learn the reversal. These results may be due to the layout of the maze: this consisted of a straight alleyway from which two arms ran off to the left—one half way down, the other at the end. Because of the different lengths of these two arms, it was in fact the further of the two that provided the shorter path to food during initial training (and therefore the nearer that was shorter during reversal). Thus the rats were trained to run past the first left turning and to select the second. It is possible that the longer the animals were trained to run straight past this first turning, the less they noticed it, so that by the end of overtraining, their behaviour was no longer under control of the choice-point stimuli. The suggestion is this: early in training animals responded in terms of external, visual cues (they noticed the first turning); as training proceeded, however, the running habit was gradually transferred to internal, proprioceptive cues. In order for successful reversal learning to be achieved, external stimulus control of behaviour had to be re-established. Thus overtraining retarded reversal.

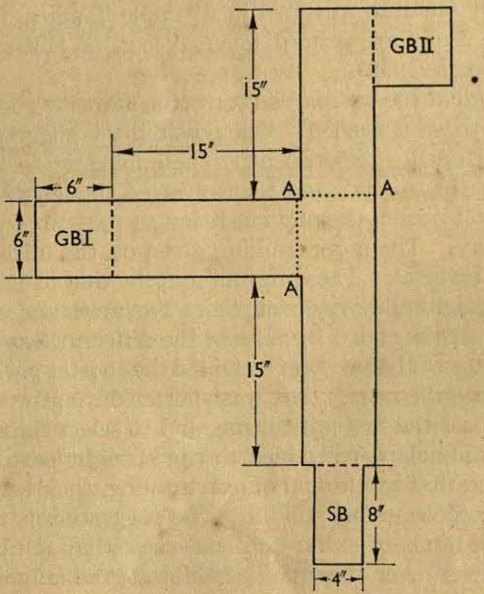
Conceivably a similar transfer to proprioceptive control tends to occur in T-maze experiments, and this would then explain why other investigators have found that overtraining can retard position reversal. In this paper, three experiments to test this hypothesis are reported. The first repeats Krechevsky and Honzik's with certain modifications to eliminate alternative explanations. The second provides a more straightforward test of the present explanation of their results. The third was designed to discover whether transfer to proprioceptive control would also occur in an ordinary T-maze.

EXPERIMENT I

In addition to the above suggestion, there are at least two other possible reasons why rats learned a reversal more slowly after overtraining in Krechevsky and Honzik's experiment. First, the correct training response was to select the second left turn; it is known that rats tend to anticipate the goal turn in a maze; if one assumes that overtraining diminishes this tendency, then their results are explained: non-overtrained animals began reversal with a stronger tendency to select the first arm than did overtrained animals. Secondly the discrimination the rats had to learn was between two paths, both of which led to food (although after different delays); this means that their situation is not strictly comparable to normal studies of T-maze reversal: overtraining might have different effects on reversal in the two situations.

The purpose of the first experiment was to repeat Krechevsky and Honzik's experiment with sufficient modifications to eliminate these two possible explanations, thus testing the importance of the hypothesis suggested in the Introduction. The maze used is shown in Figure 1. To turn the problem into a more normal position discrimination, the two goal-boxes were equidistant from the choice-point, and only one contained food. To test the first possibility, the second goal-box turned off *right* from the alleyway.

FIGURE 1



Floor-plan of the maze used in Experiments I and II.

On the assumption that after overtraining control of the maze habit is transferred to proprioceptive stimuli, it was predicted that the more training rats were given with GB II positive, the longer it would take them to reverse to GB I. As a further control, a second group of rats was initially trained with GB I positive; for this group, reversal to GB II should be unaffected by overtraining.

METHOD

Subjects

The subjects were 32 male hooded rats. They were 2½–3 months old and had previously been used in a generalization experiment.

Apparatus

The floor of the maze was made of ¾ in. chipboard, and the sides from 6 in. strips of hardboard. The metal start-box was painted black and had a transparent Perspex lid; the rest of the maze was unpainted, and was enclosed with ⅓ in. wire mesh. The entrances to the two goal-boxes were covered with white curtains, and each goal-box contained a a 2 × 1¼ × ½ in. feeding box.

The maze stood on a table in a windowless room; the only illumination was from a single 25w. lamp placed 18 in. directly above the centre of the maze.

Procedure

The subjects were accustomed to a feeding schedule of 1½ hr. food daily a week before the start of the experiment; water was always available in the home cages.

One day's pretraining was given in the maze, consisting of 10 rewarded trials, of which a sufficient number was forced (by placing a block at the beginning of one of the goal-arms) to ensure an equal number of choices of each arm. Both goal-boxes contained food.

Subjects were then divided into two groups balanced for preferences displayed during pretraining. Group C was trained with GB I positive, Group E with GB II positive.

Throughout training 20 noncorrection trials were given daily; a correct choice was rewarded with 10 sec. eating in the goal-box; after an incorrect choice (the criterion for which was that the rat should push its nose through the curtain) the subject was detained in the goal-box for 5 sec. The inter-trial interval was 2–3 min., during which subjects were kept in individual detention cages.

All subjects were given two days of initial training; on the second day no subject made more than two errors. Both groups were then subdivided; half were reversed the following day; half were given 100 overtraining trials before reversal. The criterion of reversal learning was 19 correct responses over any 20 consecutive trials.

RESULTS AND DISCUSSION

The results are shown in Table I. Overtrained and non-overtrained subgroups were closely matched for the number of errors made during initial training. Group C made more errors than Group E ($t = 2{\cdot}78$, $d.f. = 30$, $p < 0{\cdot}01$).

The measure given for reversal learning is number of errors to criterion. In Group C it is clear that overtraining had a negligible effect on reversal; but in Group E overtraining retarded reversal ($t = 2{\cdot}46$, $d.f. = 14$, $p < 0{\cdot}05$).

TABLE I

RESULTS OF EXPERIMENT I

		Initial training	*Overtraining*	*Reversal*
Groups		*Total errors*	*Total errors*	*Errors to criterion*
Control	Non-overtrained ..	3·37	—	3·75
	Overtrained ..	3·25	4·50	4·00
Exp.	Non-overtrained ..	2·12	—	6·00
	Overtrained ..	2·00	2·25	17·00

Thus in spite of the extensive differences between the two experiments, the results for Group E show the same effect of overtraining on reversal as was shown in Krechevsky and Honzik's experiment. Because of these differences (and because of the results of Group C, where overtraining did not affect reversal), it seems reasonable to rule out the two alternative interpretations of Krechevsky and Honzik's results mentioned above. The suggestion that in this situation overtraining causes animals to run past the first turning without noticing it, is not of course proved by these results, but it remains a possibility worth further investigation.

EXPERIMENT II

If this explanation is correct, then any procedure which prevents animals from running unhindered past the first turning in the maze might be expected to bring the reversal score of Group E_{100} into line with the scores of the other groups. In order to achieve this, "hurdles" were placed across the maze at the two points marked A . . . A in Figure 1. These were made high enough (3½ in.) to ensure that the animals had to pause to climb and squeeze themselves through the narrow gap between the top of the hurdle and the wire mesh maze cover. Experiment I was repeated with this single change, and it was predicted that all four subgroups would now learn the reversal at closely comparable rates.

METHOD

Subjects

The subjects were 32 male rats of the same age and from the same stock as those used in Experiment I.

Apparatus and Procedure

The hurdles were made of hardboard and were fitted into the maze by means of slots cut into narrow battens glued to the side walls of the maze. Four pairs of hurdles were made: ½, 1, 2 and 3½ in. high. During the 10 pretraining trials, subjects were given two trials with no hurdles, and two with each of the four hurdles in ascending order. Throughout training and reversal the 3½ in. hurdles were used.

All other details of the apparatus and training procedure were exactly the same as in Experiment I.

RESULTS AND DISCUSSION

The results are shown in Table II. The training scores are similar to those of Experiment I: again Group E is superior to Group C, although this time the difference is not significant ($t = 1{\cdot}55$, $d.f. = 30$, $p > 0{\cdot}10$).

It is clear that the reversal scores support the prediction made: all four subgroups show closely comparable rates of reversal; the present Group E_{100} reversed significantly faster than Group E_{100} of Experiment I ($t = 2{\cdot}90$, $d.f. = 14$, $p < 0{\cdot}02$).

The only difference between Experiments I and II was the introduction of the hurdles in the second experiment. In Experiment I, for animals trained to run past the first turning, overtraining retarded reversal; for the comparable group of Experiment II, which had to stop to climb a hurdle at the choice-point, overtraining did not retard reversal. It seems reasonable to conclude that it was the opportunity to run unhindered past the correct reversal turning that retarded reversal for the first overtrained group, that, in effect, they came during the course of overtraining to run straight past the turning without noticing it.

TABLE II

RESULTS OF EXPERIMENT II

Groups		*Initial training*	*Overtraining*	*Reversal*
		Total errors	*Total errors*	*Errors to criterion*
Control	Non-overtrained	3·25	—	3·12
	Overtrained	2·87	2·37	5·00
Exp.	Non-overtrained	2·37	—	5·12
	Overtrained	2·25	0·87	4·12

Thus the suggestion that overtraining in a maze tends to cause rats to shift from external to proprioceptive control seems to be the correct explanation for the results of Krechevsky and Honzik's experiment. In the Introduction it was argued that this account would help to explain more recent results on T-maze reversal: it is important therefore to discover whether direct evidence of this transfer to proprioceptive control can be obtained in a normal T-maze.

Experiment III

Whether a maze habit is controlled by visual or by proprioceptive stimuli was one of the points at issue in the recent "place vs. response" controversy (see Restle, 1957). One of the ways devised for deciding the issue, was to train rats in a T-maze, and then to give test trials with the maze rotated through 180°. This puts the two types of cue into opposition, and their relative importance is easily assessed. In the present experiment three groups of rats were trained for 20, 100 or 200 trials in a T-maze; the maze was rotated, and test trials were given. It was predicted that place choices would predominate in the 20 trial group, but would increasingly give way to response choices in the 100 and 200 trial groups.

Method

Subjects

The subjects were 36 male rats of the same age and from the same stock as those used in the previous experiments.

Apparatus

To a great extent the chances of the above prediction being confirmed depend upon the type of maze used. An enclosed maze placed in totally homogeneous surroundings may not offer any place cues at all (e.g. Scharlock, 1955); while it is no doubt possible to go to the opposite extreme and provide a situation with such dominant place cues that response cues would never be of more than negligible importance. In an attempt to avoid these extremes, an enclosed maze with relatively marked extra-maze place cues was used. The maze was made of the same materials and of the same cross-section as the maze used in Experiments I and II. The start-arm and both goal-arms were 21 in. long; the first 8 in. of the start-arm served as a starting box. The goal-boxes were 9 in. square. Since the two goal-arms and goal-boxes were constructed of the same materials, there were minimal intra-maze place cues. The extra-maze cues were provided by setting the maze on a table with the goal-arms at right angles to a window (facing north). The experiment was run in the morning, and the window was the only source of light in the room. Half the subjects in each group were trained to turn right, towards the window; half were trained to turn left, away from the window. Between trials, subjects were kept in individual detention cages placed behind the starting box; the experimenter stood between the detention boxes and the start-arm, and was able to run the experiment with a minimum of moving about. For the test trials, the maze was rotated through 180° so that the start-arm was now on the far side of the maze from the detention boxes.

Procedure

In most respects the procedure followed that of Experiments I and II. Subjects were maintained on a feeding schedule of 1½ hr. food per day. Pretraining was different: the subjects were simply given 30 min. exploration of the whole apparatus in groups of four the day before training began. Twenty noncorrection training trials were given daily with a 2–3 min. intertrial interval. Correct choices were rewarded with 10 sec. eating; after incorrect choices subjects were detained in the goal-box for 5 sec.

Test Trials

Five test trials were given: to Group 20 (12 subjects) immediately after their 20 training trials on Day 1; to Group 100, after their training trials on Day 5; to Group 200, after their training trials on Day 10. For the test trials the maze was rotated, both feeding boxes contained food, and subjects were allowed to eat for 10 sec. whichever choice they made. Otherwise the procedure was the same as for training.

Subjects were run in groups of six. Within each group run at the same time, half were trained to turn right, half to turn left. This is of some importance, since if all subjects run together were trained in the same direction, a temporary trail would presumably be set up leading to the correct goal-box, the problem would be soluble in terms of this olfactory cue, and (since for the test the whole maze was rotated) an apparent "response" choice in the test might merely indicate olfactory control.

RESULTS AND DISCUSSION

The results are given in Table III. The three groups were closely matched for performance over the first 20 training trials. The test results show a significant trend from place learning (Group 20), through an intermediate stage (Group 100) to response learning (Group 200). The difference between the three groups in the proportion of Trial I place choices was evaluated by a χ^2 test, and was significant at the 0·05 level ($\chi^2 = 6{\cdot}24$, $d.f. = 2$). An analysis of variance was performed on the overall number of place choices for the three groups, and showed differences significant at the 0·01 level ($F = 8{\cdot}11$, $d.f. = 2/33$). Differences between pairs of groups were evaluated by t-tests: for Group 20 vs. Group 100, $t = 2{\cdot}20$, $d.f. = 22$, $p < 0{\cdot}05$; the difference between Group 100 and Group 200 fell short of significance ($t = 1{\cdot}79$, $0{\cdot}10 > p > 0{\cdot}05$); for Group 20 vs. Group 200, $t = 3{\cdot}98$, $p < 0{\cdot}001$.

TABLE III

RESULTS OF EXPERIMENT III

Groups	Errors on training trials 1–20	Testing: Percentage of subjects making place choice on trial 1	Testing: Average number of place choices on trials 1–5
20	3·33	75·00%	3·83
100	3·16	41·66%	2·83
200	3·00	25·00%	1·92

A possible objection to this experiment is that a subject may have made a "response" choice even though responding to external cues. For the test trials, the whole maze was rotated; thus a subject making its training response is still approaching the same goal-box as during training, even though it is running to the opposite side of the room. Although a homogeneous maze was used, it remains possible that there were some intra-maze cues, and that response choices were in fact controlled by such cues. To check on this possibility, 16 further rats were run in the same apparatus, either for 20 or for 200 training trials, and then given a single test trial. On this test trial, the goal-arms and goal-boxes were left in position, while the start-arm was moved to the opposite side of the maze. The maze had been modified by providing a detachable section in the wall of the goal-arms opposite the original entrance from the start-arm; on the test trial, this section was removed and fitted to the other side of the maze.

In this situation, both intra- and extra-maze cues are in combination, and would dictate a place choice on the test trial; a response choice can only occur if the habit is no longer under external control. All 8 subjects given 20 training trials made place choices on the test trial, whereas 6 out of 8 subjects given 200 training trials made response choices. This difference is significant at the 0·01 level ($\chi^2 = 9{\cdot}60$).

These results, therefore, provide direct evidence to support the contention that rats may solve a maze problem initially in terms of external stimuli, that with further training the habit is increasingly controlled by proprioceptive stimuli, and that eventually the habit may become automatic and partially independent of the initially important external stimuli. It should be pointed out first that this is by no means an original suggestion, and secondly that there is some earlier evidence in its favour.

Carr (1917) claimed that "the white rat learns the standard type of maze primarily in tactual and kinaesthetic terms, that during the course of learning the control is gradually transferred from contact to kinaesthesis, and that after the problem is thoroughly mastered the act is to be regarded as a kinaesthetic-motor coordination with an occasional reliance upon contact in times of emergency" (p. 259). A similar claim had earlier been made by Vincent (1915). The evidence for the assertion was at the time negligible—for example, the most impressive demonstration of the role of proprioception in maze running (Carr and Watson's 1908 study on shortening or lengthening the units of a maze) had involved rats that had received extensive training. Two more recent experiments, however, lend some support to the suggestion. Macfarlane (1930) studied the effects of giving rats different numbers of trials running a maze before transferring them to swimming through the maze (or vice-versa). He found no increase in error scores after four trials of initial training, a slight increase after 12 trials, a rather larger increase after 33 trials. It would be largely proprioceptive cues that were affected by this change; the results thus imply an increasing role of such cues as training continues. Kendler and Gasser (1948) gave rats various numbers of trials in a T-maze, and then tested their general orientation towards the goal (and presumably this would be controlled by external stimuli) in the manner devised by Tolman, Ritchie and Kalish (1946). They found significantly better orientation after 20 trials of T-maze training than after 100.

General Discussion

It is now possible to apply the concept of transfer to proprioceptive control in greater detail both to the present results and to those of other position reversal studies. It is clear that the rate at which such transfer occurs, and even the extent to which it occurs at all, will be critically influenced by the design of maze used: in an enclosed straight alleyway, the change will presumably be rapid and complete; in a T-maze where a definite turn at a definite point must be made, less rapid and less complete. In Experiment I, the correct solution for Group C involved turning left at a choice-point: this is rather similar to a normal T-maze problem; Group E merely had to run to the end of the alley and turn right: this is rather similar to the normal straight alleyway problem. Thus Group E would be more likely than Group C to change over to proprioceptive control; with the further assumptions that successful reversal learning demands the re-establishment of external control, and that the introduction of the hurdles in Experiment II tended to prevent the formation of an automatic response chain, the results of the first two experiments fall into place.

Nevertheless the results of Experiment III indicate that this change in stimulus control *does* occur in an orthodox T-maze (the earlier evidence supporting this idea was also obtained in single- and multiple-unit T-mazes); so if this change does retard reversal learning, we have a further reason (in addition to the absence of irrelevant cues noted above) for the general failure to find the overtraining reversal effect in position reversal experiments, and would even expect overtraining slightly to retard reversal—as is indeed the case. The extent to which overtraining would retard reversal would depend on the degree to which non-overtrained animals were still responding to external stimuli and overtrained animals were responding in terms of proprioceptive stimuli, and this would depend on various features of the experimental situation. Thus different experimenters would tend to get different results, and again this is so: in a total of four experiments, D'Amato and Jagoda (1962) never found a significant difference between their overtrained and non-overtrained groups, while Hill *et al.* (1962, 1963) found that overtraining significantly retarded reversal in two out of four experiments.

This analysis will also help to explain the apparently contradictory findings of those who have shown that position reversal, even in the absence of irrelevant cues, may be facilitated by overtraining. There are three experiments in this category—Capaldi (1963), Ison and Birch (1961), and Bruner *et al.* (1958). But Capaldi used an elevated maze; an elevated maze differs from an enclosed maze chiefly in the vastly greater importance of external, visual stimuli in the control of the habit (Tsang, 1934; see also the review of the recent place vs. response experiments by Restle, 1957); it is probable therefore that transfer to proprioceptive control never occurred in Capaldi's situation. Ison and Birch precluded the possibility of such transfer occurring, since they trained their animals merely by *placing* them in the positive and negative goal-boxes. Finally, in the experiment by Bruner *et al.* (1958) where rats learned an alternating habit in a 4-unit maze, response chaining might actually have facilitated reversal, since with a chained sequence only the first choice would have to be reversed, while non-overtrained animals would have to reverse their response at each of the four choice-points.

This relatively simple account, therefore, succeeds in explaining all the available data on position reversal. It seems worth showing that it may also be able to explain the results of another set of experiments—those on the effects of overtraining on extinction. North and Stimmel (1960) and Ison (1962) have shown that the more training rats are given on a simple alley running response, the faster the response is subsequently extinguished. This might be because responses controlled by proprioceptive stimuli extinguish faster than responses controlled either by external stimuli or a combination of the two. This of course is a merely speculative notion, but it will again serve to reconcile apparently conflicting results, by showing why this effect of overtraining on extinction does not always occur. Lawrence and Festinger (1961) used a rather complex alleyway, with several turnings, hurdles and swing doors, and found extinction unaffected by overtraining. The results of Experiment II show that hurdles are quite effective in preventing transfer to proprioceptive control. Lawrence and Miller (1947) using an elevated maze found that rats given 16 acquisition trials extinguished more slowly than rats given only 8 trials; whereas Ison using an enclosed maze with a relatively homogeneous extra-maze environment found that rats given 20 acquisition trials extinguished faster than rats given 10. It was suggested above that an elevated maze discourages, and an enclosed encourages transfer to proprioceptive control. Finally it is well established that increasing the number of reinforcements in a Skinner box does *not* decrease subsequent resistance to extinction (Williams, 1938; Harris and Nygaard, 1961; Margulies, 1961; D'Amato, Schiff and Jagoda, 1962); and of course transfer to proprioceptive control is unlikely to occur in a Skinner box.

It is worth making one final point. The fact that overtraining frequently leads to faster reversal learning, and also frequently leads to faster extinction, has made it seem natural to interpret both effects as due to a common process. (Capaldi and Stevenson (1957) would explain both in terms of the discrimination hypothesis of extinction; Amsel (1962) invokes the part played by frustration in extinction as the explanation of both.) The present analysis implies the exact opposite—that response chaining causes overtraining to facilitate extinction, but to retard reversal. Whether or not the analysis is correct, a careful examination of the variety of situations used in these experiments supports at least part of this contention. In a simultaneous visual discrimination, overtraining facilitates reversal, but retards extinction (Mackintosh, 1963*b*); in an enclosed maze, it retards reversal but facilitates extinction; in an elevated maze, it facilitates reversal but retards extinction. In general, therefore, the two effects appear in entirely different experimental situations, and cannot be due to the same cause.

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STUDY OF THE SINGLE CHANNEL HYPOTHESIS AND INPUT REGULATION WITHIN A CONTINUOUS, SIMULTANEOUS TASK SITUATION

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Subjects performed simultaneously on an auditory tracking and an auditory discrimination task, with each task presented to a separate ear. Information transmitted on the tasks was measured as a function of ability to predict task characteristics, input information-rate, and input discriminability. Based on comparison of single- versus simultaneous-task performance, support was found for a single, central decision-type channel in information processing, having as one primary limit the rate at which information can be accepted. Discriminability of inputs also was found to be a limit on information processing rate. Although ability to predict a task's characteristics facilitated performance on that task, in this experiment it did not result in facilitation of performance on the second task. Relevance of these findings to certain aspects of Broadbent's information-processing model is discussed.

The term "single channel hypothesis" generally has designated the concept of a central decision or translation mechanism in human perceptual-motor functioning. A major proponent of the single-channel hypothesis within an information processing model of human performance has been Broadbent (e.g. 1958), who refers to the single channel as a "limited capacity channel." Within a given task, the primary limit is the rate at which the channel can process information. Information arriving at the channel may be accepted only after some delay if the channel already is occupied with the processing of some prior input. Broadbent also suggests that the channel may be additionally limited by the need to monitor output (as well as to select input). Similarly, Welford (1960) has stressed that the limiting factor in the speed of a perceptual-motor operation is the need for decisions by the central-mechanisms about the classes of signals occurring and about the selection of appropriate responses.

In this paper, the feasibility of the single-channel hypothesis is examined within the framework of a continuous auditory pursuit tracking task (called the T-task) presented either alone or in conjunction with a discrete-type auditory discrimination task (called the D-task). Generally, some performance decrement from single- to multiple-task situations is to be expected, although the type and locus of this decrement is not *a priori* apparent. We are interested in examining, however, the suggestion from Broadbent's work that a greater performance decrement is to be expected in a multiple-task situation, if a particular task is of relatively high information rate than if it is of relatively low information rate. Following Broadbent's view, higher information rates should result in more frequent queueing of inputs at the channel bottleneck. In this study, the T-task is used as the information-rate variable, with D-task information-rate being held constant.

Broadbent's information processing model (Broadbent, 1958, p. 297) is elaborated to include feedback control of a pre-channel input selection mechanism (the selective filter) by the post-channel long term storage mechanism. The ability to predict the input characteristics of a set of signals comprising a task (i.e. perceptual anticipation (Poulton, 1952)) would in Broadbent's view, "make the task independent of the objective occurrence of external events. It would also make it more possible to combine the task with other tasks, since reduction of the total demand on the . . . [limited capacity channel] would allow other items of information to pass through it" (Broadbent, 1958, p. 286). Thus it is implied that the long term storage mechanism, given predictable inputs, can serve as a control on the rate at which input signals are selected. Examination of this suggested feedback control effect is attempted within this study. Only one of two groups of subjects is given training on the statistical characteristics of the T-task (through repeated observation of the course). With these characteristics held in long term store, presumably much of the required output for the T-task might be generated without direct input observation, thereby freeing the limited capacity channel for additional input from the D-task. From this view, one would expect performance on the accompanying D-task to be superior for those subjects able to predict the statistical characteristics of the T-task.

In addition, two supporting variables are investigated in this study: (1) short periods of interruption of T-task input signals, for their effects on T-task performance and (2) different levels of D-task discriminability, for their effects on both D- and T-task performance. The interruption-noninterruption variable is used to provide indication of the degree to which predictability of the T-task is achieved. The variable of discrimination level is introduced to permit testing of the generality of any observed effects of information rates in the dual-task situation. As pointed out by Leonard (1955) discrimination level may have an effect on information processing rate independent of the associated information level of the task.

METHOD

Description of tasks

Monotic, auditory pursuit tracking (*T-task*). During repeated, identical 53-sec. trials, subjects tracked a programmed course, consisting of a 1000 cps tone varying in intensity, with a 500 cps tone whose intensity was under their own control. Both course and track input were to the subject's right ear.

There were two unique courses: (1) a "high" information course, with an information rate of 1·8 bits/sec., or approximately 95 bits/trial and (2) a "low" information course of 1·1 bits/sec., or approximately 58 bits/trial. Techniques by which these courses were constructed are given in Herman (1961). The courses were tape-recorded and were of 53-sec. duration. Since each course had a maximum intensity range, over the same set of amplitude values, of approximately 35 db, approximately the same amount of manual knob rotation of the gain control of the following oscillator was required for tracking each course. A somewhat faster rate of rotation was required, however, for following the course of higher information rate.

Discrimination of auditory durations (*D-task*). Subjects were required to identify through a verbal response the longest pulse in a series of five tonal pulses. The tonal pulses (four equal and one unequal in duration) were presented to the subject's left ear at either 350 cps or 2250 cps. The lower frequency represented a relatively easy temporal discrimination; the higher frequency a relatively difficult temporal discrimination.

The duration of a single presentation of a tonal series was approximately 7·5 sec. For both the 350 and 2250 cps tonal pulse series, the four equal pulses were 1·1 sec. in duration; the unequal pulse for the 350 cps series was 1·6 sec. in duration and for the 2250 cps series, it was 1·3 sec. Inter-pulse interval was always 0·53 sec. in duration. The ordinal position of the unequal pulse within the series of five was varied randomly. The average input entropy for each pulse in a series of five was approximately 2·3 bits; there were therefore a total of 23 bits in the two series of five pulses occurring during a single tracking trial.

Combining the tasks. Table I shows the dual-task conditions assigned to each of four groups. In addition to the conditions shown, during each occurrence of a series of five tonal pulses, the course input for the T-task either remained on or was interrupted. If input was interrupted, the subject was required to track the predicted course. His following tone, or output, was never interrupted. Since there were two occurrences of a tonal-pulse series during each of the last 20 test tracking trials, there was possible either zero, one, or two interruptions of input of the T-task per 53-sec. trial. Four of the 20 trials had no interruptions, 12 had one interruption, and four had two interruptions. These interruption levels were randomly assigned to the successive trials.

TABLE I

SUMMARY OF EXPERIMENTAL DESIGN

(n = 10)

	Tracking task (Right ear)			*Discrimination task (left ear)*
Group	*No. of training trials*	*Relative information content*	*No. of test trials*	
PH	72	High	22	All groups: two series of five tonal pulses during each test trial from 2–22; half of series "easy" and half "difficult."
PL	72	Low	22	
UH	0	High	22	
UL	0	Low	22	

P = Practiced; U = Unpracticed; H = High information; L = Low information.

Apparatus

Each subject was trained and tested individually in an anechoic chamber at the Pennsylvania State University. Task signals and instructions were received via a PDR-8 headset. A Beltone audiometer, set at 500 cps, was positioned on a table in front of the subject. The subject manipulated the gain control of this audiometer through a masking sleeve, which obviated use of visual cues for the T-task.

An Ampex dual-channel tape recorder (Model 970), located in an adjacent control room provided the task signals. One channel provided D-task signals to the subject's left earphone while the other presented T-task signals to the right earphone. The subject's tracking output from the audiometer also was fed to his right earphone. The tracking output was directed to a Krohn-Hite band pass filter (Model 330–M), set to pass frequencies between 350–750 cps only, and subsequently to a Bruel and Kjaer level recorder (Model BL–2304), which provided an analog record of the tracking output.

Procedures

Training phase. The 72 training trials indicated in Table I for groups PH and PL consisted of four consecutive days of tracking T-task signals alone, 18 practice trials per day. These subjects did not encounter the D-task until the subsequent test trials.

On day 1, tracking was accomplished by auditory reference alone. Immediately following the day 1 tracking, subjects were familiarized visually with the course pattern through observation of the graphic output of the stimulus course. Each subject was provided with a card displaying the pattern of the course he was tracking, and was requested to take it home and study it.

On days two and three, the subject was allowed to refer to this card during tracking. At the beginning of day four the cards were collected and the subject tracked only by auditory (and perhaps kinesthetic) reference.

Test phase. This consisted of 22 consecutive tracking trials and 40 simultaneous tonal pulse discriminations; two tonal series occurred per each of the last 20 tracking trials. Trials 1 and 2 consisted of the tracking task alone. The 20 easy and 20 difficult discrimination series were randomly ordered among and within the last 20 trials.

Immediately prior to the test phase, unpracticed subjects (groups UH and UL) were provided ample opportunity to familiarize themselves with the relatively simple characteristics of the gain control and the output of their following oscillator. They also practiced tracking two trials of the T-task alone and two additional trials of the T-task in combination with the D-task, under both interrupted and non-interrupted conditions. (Subjects previously practiced on the T-task immediately prior to trial 1 of the test phase tracked only the two trials containing both the T- and D-tasks.) All subjects were told that both tasks were equally important and were instructed to attempt to perform as well as possible on *both* tasks during simultaneous occurrences.

Scoring. For each tracking trial, the amplitude range of the subject's track was quantized into eight equal intervals. These quantized response amplitudes then were compared with the previously determined course amplitudes at *related* sample points (i.e. adjustments were made for lead-lag errors (Crossman, 1960)). The information transmission score for the trial was computed from the resulting stimulus-response matrix. Using related sample points and a consistent number of amplitude levels ensured that the subject's ability to transmit the course information was being measured accurately and consistently (i.e. errors in transmission of course information were not confounded with the subject's lead-lag biases or with his level of ability in reproducing the exact loudness level of the course).

Information transmitted on the D-task was computed from an S-R matrix comprised of the five stimulus pulses and the subject's corresponding responses (as to the ordinal position of the longest pulse in a series). Group scores on this task, as on the T-task, were obtained by averaging over subjects.

Subjects

Forty male undergraduate students at the Pennsylvania State University served as subjects. It was required that subjects have no known hearing defect or loss in either ear; hearing was not tested but subjects were questioned carefully on this point.

RESULTS

One subject from each group was dropped from the following data analyses. Two were dropped because of tracking equipment malfunction occurring during the test phase; the remaining two were excluded by random procedures in order to simplify the extensive data analyses. All effects reported as significant have probability of occurrence under the null hypothesis of 0·5 or less.

Single versus multiple-task performance levels

Table II compares the mean number of bits/trial transmitted by each group on the T-task during nonsimultaneous and simultaneous conditions. The nonsimultaneous score for the two practiced groups was the mean number of bits/trial transmitted during trials 2, 5, 8, 11, 14, and 17 of the final 18 training trials; for the two unpracticed groups, the nonsimultaneous score was the number of bits transmitted during trial 2 of the 22 test trials. (It was intended originally that the mean of test trials 1 and 2, two noninterrupted trials during which the D-task did not occur, be used as the measure of nonsimultaneous tracking performance for unpracticed groups; however, performance scores on trial 1 were reduced artificially because of the subject's initial acquiring movements. That is, at the start of the test trials the subject's loudness control knob was set at zero gain, his first operation was to "lock-in" on the target by adjusting the gain to at least the minimum level of the course. This operation resulted in an artificially high initial lag error for test trial 1.)

For all groups, the simultaneous score was the mean number of bits/trial transmitted during test trials 3–22.

A 3-way analysis of variance was conducted on these T-task transmission scores. There were highly significant main effects of practice levels, information levels, and a significant overall decrease in transmission occurring from the nonsimultaneous to the simultaneous condition. The finding of major importance for the single-channel hypothesis, however, is a significant information-simultaneity (I × S) interaction. Essentially this significant interaction indicated a real difference between high and low information groups in the magnitude of decrement from nonsimultaneous to simultaneous tracking. This difference was substantiated through tests of simple effects for this interaction, in which a highly significant decrement was found for the high information groups but a nonsignificant decrement occurred for the low information groups. Since the I × S interaction pools the effects of practice and no-practice, the significant interaction effect obtained is clearly attributable to the two levels of the information and of the simultaneity variables. It appears, therefore, that the information-rate limited, single-channel concept, is given additional support by these findings.

TABLE II

THE MEAN NUMBER OF BITS/TRIAL TRANSMITTED ON THE TRACKING TASK DURING NONSIMULTANEOUS AND SIMULTANEOUS CONDITIONS

(n = 9)

Group	*Nonsimultaneous*			*Simultaneous*		
	M	*SD*	% *Input*	*M*	*SD*	% *Input*
PH	40·47	4·78	42·60	35·56	5·08	37·43
PL	31·26	1·62	53·90	30·03	1·66	51·78
UH	37·72	4·02	39·71	31·67	1·79	33·34
UL	28·19	7·01	48·60	26·34	2·62	45·41

% input is the ratio of transmitted information to input information; i.e. T (in; out)/H (in).

Filter control by the long term storage mechanism

Table III shows the mean number of bits/trial transmitted by each group on the combined conditions of the D-task (i.e. summing over easy and difficult levels). It can be seen that the results do not give support to Broadbent's suggestion that perceptual anticipation should facilitate dual-task performance, since each unpracticed group transmitted more information on the D-task than did the corresponding practiced group.

TABLE III

THE MEAN NUMBER OF BITS/TRIAL TRANSMITTED ON THE DISCRIMINATION TASK

(n = 9)

Group	*M*	*SD*	% *Input*
PH	13·16	3·44	57·22
PL	13·95	4·16	60·65
UH	14·88	4·09	64·70
UL	17·63	2·40	76·65

We must consider, however, the possibility that prediction of input was not being employed (or, at least, employed effectively) by practiced groups. To examine this contention, the performance achieved on the T-task by each group during four trials when course input was interrupted each time the D-task occurred was compared with performance during four different trials when the course input remained on during each occurrence of the D-task. The mean performance levels for each group during each of these conditions is shown in Table IV. An analysis of variance of these T-task scores indicated that the overall main effect of interruption levels was not significant across the four groups nor was the information-interruption interaction, but there was a significant practice-interruption interaction, which comprises the crucial test of effective employment of predictability. Tests of simple effects for this latter interaction indicated that for unpracticed groups significantly more information was transmitted during *non*interrupted conditions, but for practiced groups the difference was nonsignificant, with the greater mean actually occurring for the interrupted condition. Further simple effects tests across groups revealed that transmission scores of practiced groups significantly exceeded those of unpracticed groups during interrupted trials but did not during noninterrupted trials. From these results, it is clear that we can reject the prediction hypothesis for the unpracticed groups but cannot for the practiced groups. The fact that 53-sec. trials containing two interruptions of approximately eight sec. each were tracked with virtually no decrement by practiced groups, as compared with noninterrupted trials, would appear to be strong evidence that they were able to employ prediction effectively on the T-task.

TABLE IV

THE MEAN NUMBER OF BITS/TRIAL TRANSMITTED ON THE TRACKING TASK DURING TRIALS CHARACTERIZED BY EITHER INTERRUPTIONS OR NONINTERRUPTIONS OF INPUT

(n = 9)

Group	*Interrupted*[a]		*Noninterrupted*[b]	
	M	*SD*	*M*	*SD*
PH	36·40	5·95	34·89	5·66
PL	30·03	3·14	30·07	2·14
UH	30·66	2·99	31·65	3·88
UL	23·99	2·36	28·22	2·64

[a] Based on tracking trials 3, 12, 16, and 20.
[b] Based on tracking trials 6, 10, 13, and 22.

(Examination of individual tracking records revealed that tracking movements did not cease nor appear to change appreciably for any subject during D-task occurrences, obviating the possible criticism that subjects just were not doing any tracking at all during D-task occurrences.) It cannot be concluded from these results, then, that the apparent effective use of prediction by practiced subjects on the T-task resulted in any increase in available channel capacity for the D-task input.

Effects of interruption levels of the T-task on D-task performance

The preceding finding of a significant practice-interruption interaction for T-task performance suggested an investigation of effects of interruption of the T-task on D-task performance. That is, could it be considered that practiced groups were neglecting D-task input during interruptions of the T-task, thereby in part accounting for their effective T-task but ineffective D-task performance during interruptions?

The information transmitted on the two levels of the D-task as a function of interruption levels of the T-task is shown in Table V. An analysis of variance based on these scores indicated that the main effect of T-task interruption levels on D-task scores (combining both easy and difficult conditions) was not significant. Thus, whether T-task input was interrupted or not had no effect on overall D-task performance.

The general conclusion of importance from this analysis, and from the preceding one, is that although practiced groups predicted T-task input significantly better than did unpracticed groups, they did not perform in any different manner from the unpracticed groups on the D-task during interrupted and noninterrupted periods.

TABLE V

THE MEAN NUMBER OF BITS/TRIAL TRANSMITTED FOR EACH LEVEL OF THE DISCRIMINATION TASK DURING INTERRUPTED AND NONINTERRUPTED OCCURRENCES OF THE TRACKING TASK

(n = 9)

	Easy				*Difficult*			
	Interrupted		*Noninterrupted*		*Interrupted*		*Noninterrupted*	
Group	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
PH	16·79	1·90	18·90	2·54	15·86	3·13	14·89	1·92
PL	19·08	3·78	19·35	2·08	15·12	3·55	14·17	4·03
UH	19·26	3·72	20·08	1·62	16·24	5·02	14·40	3·55
UL	20·21	2·18	21·69	1·08	17·72	5·29	17·90	2·37

Each mean score is based on 90 discriminations (9 subjects × 10 responses each).

For example, no two-way interactions between interruption levels and either practice levels or information levels was found to be significant. It cannot be said, then, that practiced groups achieved better prediction on the T-task at the expense of D-task information.

Effects of discrimination levels of the D-task on D- and T-task performance

Table VI shows the mean transmission scores on each of the two tasks as a function of the discrimination levels of the D-task. A Walsh nonparametric test of these D-task scores indicated that for each group significantly more information was transmitted for the easy discriminations than for the difficult.

TABLE VI

THE MEAN NUMBER OF BITS/TRIAL TRANSMITTED FOR THE SIMULTANEOUS TRACKING AND DISCRIMINATION TASKS DURING EASY AND DIFFICULT DISCRIMINATIONS

(n = 9)

	Tracking task				*Discrimination task*[c]			
	Easy[a]		Difficult[b]		Easy		Difficult	
Group	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
PH	35·96	5·99	35·91	5·83	17·85	2·54	14·82	2·12
PL	29·78	2·22	29·97	3·25	19·22	2·17	14·65	3·30
UH	29·96	4·08	32·73	2·19	19·67	2·17	15·32	4·18
UL	25·78	2·08	26·20	3·40	20·95	1·25	17·76	2·79

[a] Based on the mean tracking score during test trials 3, 11, 13, and 21, containing only easy discriminations.

[b] Based on the mean tracking score during test trials 7, 8, 17, and 18, containing only difficult discriminations.

[c] Scores for each group within each discrimination level are based on 180 responses (9 subjects × 20 responses).

To analyze effects of D-task levels on T-task performance, the mean number of bits/trial transmitted by each group during four trials containing only easy discriminations was compared with the mean performance during four different trials containing only difficult discriminations, as indicated in Table VI. Walsh tests within each group revealed that none of the T-task differences reached the 0·05 criterion level, indicating that difficulty level of the D-task had no significant effect on T-task performance (it is seen in Table VI that within-group means for the T-task actually are very close). Thus effects of difficulty levels on information transmission are restricted to the task in which the difficulty variable appears.

Total performance on both tasks

Comparisons of total performance of the four groups of this study is of considerable interest, since one might expect, *a priori*, differentiating effects of practice levels and information levels on total performance. For example, although evidence was not obtained for a specific facilitating effect of predictability of the T-task on D-task performance, it is still possible that predictability served to facilitate the *total* information processing performance of the subjects.

Total performance scores for each group were obtained by combining additively the mean bits/trial for the T-task during the simultaneous condition (Table II) with the mean bits/trial for the D-task (Table III). These total scores are: PH = 48·72; PL = 43·98; UH = 46·55; UL = 43·97. An analysis of variance of these scores indicated that neither the overall between-groups effect, nor the main effects of practice levels, information levels, nor their interaction reached the 0·05 level of significance. Based on *a priori* expectation of differentiating effects, pertinent individual comparisons were made of the differences in total performance between the four groups, but none of these comparisons reached the 0·05 level of significance. In this overall sense, then, the four groups performed approximately equivalently as transmitters of the total information contained in the two tasks. This appears to indicate an overall channel limitation, such that total channel capacity may be partitioned in different ways among simultaneous tasks, but the total amount of information processed is primarily a function of the information capacity of the channel rather than of the information characteristics of the tasks.

Discussion

The single-channel hypothesis, for the case of a continuous input and simultaneous task performance appears to gain additional support from the results of this study. The observation of relatively greater transmission loss during simultaneous task performance for higher information rates does appear to be well-described within the framework of a central decision channel construct exercising a "funnelling-type" action on incoming signals.

It should be recalled that an attempt was made to control peripheral effects on performance in this experiment. Thus, each task was presented to a separate ear at tonal frequencies selected so as to minimize inter-task masking effects (Fletcher, 1953). The required task outputs of continuous hand movements and of momentary vocalization would not be expected to cause peripheral motor interferences. Furthermore the amount of motor effort required for the T-task was not substantially different across information levels, since for both high and low information groups the range of required control movements of the following oscillator was approximately 35 db. Therefore, the noted significantly greater performance decrement occurring for high information, simultaneous task performance reasonably can be attributed to a central bottleneck, such as the channel proposed by Welford (1952, 1959), Davis (1957), Broadbent (1958) and others.

Although evidence was obtained indicating that practiced subjects were able to predict T-task input effectively, the conclusion could not be reached that this predictive ability enabled these subjects to increase the amount of channel capacity given to D-task input. It appears that what was learned during practice sessions was a programme of the perceptual elements of the task. Although this learned programme undoubtedly enabled the subject at times to substitute anticipation of input signals for direct observation, it did not act as a substitute for decisions about action. That is, the need to monitor responses was not alleviated for the practiced subjects since they had to continue to rely primarily on auditory feedback to ensure that their responses were in accord with intention. Thus, long term storage feedback regulation of the filter mechanism's input selections, as indicated in Broadbent's model, can not alone guarantee facilitation of dual-task performance, unless the attendant motor programme also is automated to a high degree.

The finding that for a constant input information level easy discriminations resulted in significantly more D-task information being transmitted than did difficult discriminations indicates that discrimination level must be considered as another type of limit on the channel. One cannot state simply what effect a given input-information-rate will have on information processing rate without considering as well the discrimination levels among signals. This would be in accord with a hypothesis of a decision or classification type function for the channel, signals more alike being more difficult to classify uniquely. Leonard (1955) has pointed out earlier that a smaller number of bits may be handled less effectively than a larger number, when the stimuli representing the former are less easily discriminable.

The fact that the discrimination levels of the D-task had no significant effect on T-task performance level may imply that some constant amount (depending on the individual subject) of available channel capacity was given to D-task input, regardless of its discrimination level. Note that since the occurrence of an easy or a difficult level was conveyed completely and immediately to the subject through a signal-frequency code—325 and 2250 cps respectively, for the easy and difficult levels—the subject conceivably could have taken the strategy of assigning different amounts of channel capacity to each D-task level. But this did not seem to be the case.

The results of this study are comparable to those of Griew (1959), who found greater interfering effects occurring at faster tracking speeds (i.e. higher input-information rates), in the performance of a simultaneous visual tracking and auditory RT task. In the use of information measures in the present experiment, however, a more pertinent measure (for the single-channel hypothesis) of the characteristics of the stimulus variables is obtained than has occurred in the use of rate and amplitude errors and of RT in Griew's study. In addition, the information measure permits ready combination of performance levels achieved on each of the two tasks. In this respect, a general observation from this experiment is that individuals are able to process information effectively from two simultaneous tasks. That is, a second task can be added to a first one, and although a decrement may be noted in the information processing rate for the first, the combined information processing rate attained exceeds that of the first task alone. It seems apparent, therefore, that a good part of the information processing system of the individual is able to handle inputs simultaneously (the exception of the limited capacity channel being previously noted).

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VOCALIZATION-AT-PRESENTATION AND IMMEDIATE RECALL, WITH VARYING PRESENTATION-RATES

BY

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An experiment is reported in which the subject read visually presented lists with four different degrees of vocalization; immediately after reading each list he was required to reproduce it either aloud or in writing. Each list consisted of eight consonants and presentation rates were varied between 1 and 4 letters per sec. For any given series of lists, the subject was asked either to read the letters silently, or to mouth them silently, or to whisper them, or to say them aloud while reading.

At the fastest presentation-rate immediate recall improved monotonically with the degree of vocalization during reading of the lists; at slower rates this generalization held less well, especially for the lower degrees of vocalization. Vocalization was most helpful at the highest presentation-rate.

The overall amount correctly recalled was better for more slowly presented lists and for written as opposed to spoken recall. Analysis of the errors suggested that acoustic confusions were affected by the conditions of presentation; and that serial order intrusions were independent of presentation- or recall-conditions. An apparent variation of transpositions with voicing-and-recall-method failed to reach statistical significance. Theoretical implications of the experiment are discussed, including reference to Broadbent's theory of short-term memory (1958).

INTRODUCTION

The experiment to be described is concerned with the effect of vocalization of presented stimuli upon their subsequent availability for immediate recall, the stimuli here used being visually presented consonant-lists. An attempt was made to vary the "amount of vocalization" by having the subjects either observe the consonants silently, or articulate them silently as they appeared, or whisper them, or say them aloud, the hope being to establish some sort of "vocalization-gradient." It was also considered important to study any interaction between vocalization effects and the rate at which stimuli were presented, partly in view of Broadbent's hypothesis (1958) of the existence of a single memory-channel operating just after sensory intake (or possibly nearly simultaneously with sensory intake) which could be subject to overloading. On the basis of this theory it might be surmised that vocalizing, despite its possible merit of feeding back to the subject additional information relevant to each single item, might nevertheless present too much information for easy handling at fast rates, and that therefore any recall-advantage for "vocalized" over "silent" intake of the stimuli should be less for fast as compared with slower rates.

The mode of recall was also varied. The materials used were consonants and recall was obtained either verbally or by written reproduction; data were collected for both recall-modes in order to ascertain how far any features of recall performance might be put down to the actual process of recall, resulting from factors such as the similarity between vocalizing and verbal recall, as opposed to changes occurring during storage such as interference or decay.

Amongst the relevant literature, there are several papers showing that overt vocalization of stimulus material, whether meaningful or otherwise, facilitates subsequent recall (e.g. Barlow, 1928; Kurtz and Hovland, 1953). Some of this work rose from (mainly early) studies of the relative effectiveness of visual, auditory and other modes of presentation of material to be learnt (reviewed by Day and Beach, 1950). In particular, Von Sybel (1909) used a variety of presentation-conditions and found that visual presentation with simultaneous reading aloud gave the best recall, particularly at his fastest rate (nonsense-syllables, one per sec.). Again O'Brien (1921) found that recall was best if subjects saw the material, spoke it, and wrote it down at presentation, this finding being more applicable to nonsense-syllables than to meaningful material. In a recent survey Mowbray and Gebhard (1958) assert that "it is almost universally agreed that optimum understanding and retention of prose material is obtained when the same material is presented simultaneously to both senses." It should be borne in mind however that individual modality-preferences may also play a part (cf. Meumann, 1913, p. 324; Buschke, 1962).

As regards the presentation-rate, most workers find that slow presentation is better than fast for recall. Bergström (1907), McReynolds and Acker (1959) and Mackworth (1962) specify a decreasing linear relationship between the amount recalled and log (number of items presented per sec.). However, Conrad and Hille (1958) found that a 2/3 sec. per digit rate of presentation gave better recall than a 2 sec. per digit rate, particularly for unpaced as opposed to paced recall. Posner (1963) has suggested possible explanations for this discrepancy in a recent review. But most workers agree that within a single modality subjects can accept relatively fast presentation-rates. In the present experiment a maximum rate of 4 consonants a sec. was used.

Opportunity has also been taken for a fairly detailed analysis of the type of errors made, in the hope that some distinction might be found between errors which are apparently dependent on presentation- as opposed to recall-conditions. A summary of evidence on various kinds of errors in immediate memory is given by Conrad (1959).

We may also note here the results of an experiment by Pollack (1963): varying the presentation-rate of interfering digit-sequences, he found that recall of the desired list was lower, the faster the interfering digits were presented. He also found that vocalization of the interfering list led to more retroactive inhibition, than did non-vocalization of the interfering list: 'mouthing' gave intermediate results.

METHOD

General design. Twenty-four subjects, all undergraduates or research students, were presented with lists of eight consonants stencilled onto a moving roll of paper arranged so that only one letter at a time was visible through a viewing-slit. The lists had their letters spaced at different intervals, giving presentation-rates of 1·0 letters per sec., 1·33 letters per sec., 2·0 letters per sec., or 4·0 letters per sec. Before each list subjects were instructed either to read each letter silently as it appeared (R), or "mouth" it without making a sound (M), or whisper it (W), or say it aloud (V). They were also instructed to recall the list aloud or else to reproduce the list by writing, as soon as they liked after the list ended. They could recall the letters in any order they liked but had to specify the position of each letter. These conditions will be referred to as follows: presentation-rates: 1, 1·33, 2, 4; vocalization-conditions: R, M, W, V.

The main method of scoring recall performance was to count the absolute number of letters given correctly, in their correct positions in the lists. Details of the error-analysis are given below.

Material. The material consisted of consonants because it was felt that confusions between acoustically similar letters such as B and V might be of particular interest. In compiling the *lists* care was taken to avoid obviously connotative sequences. Lists were arranged into eight divisions each containing sixteen lists, each division being stencilled in black upper case onto a *roll* of white paper. On each roll the letters were again divided into four *groups* of four lists each: each group had four lists at a single presentation-rate, the latter being obtained by appropriate spacing of the letters within the lists. On each roll there were thus four lists at each of the four presentation-rates used in the experiment.

Just before the beginning of each list was stencilled a short red line to act as a warning-signal; another short red line was stencilled just after the end of the list.

The consonants on each *roll* were so arranged that (i) each letter occurred exactly twice in each group and (ii) each letter occurred once in each position within the list (1st position . . . 8th), there being 16 lists on the roll, and the sixteen consonants being B, C, D, F, H, J, K, L, M, N, P, S, T, V, X, and Z. Precaution (i) was taken to minimize any effects of letter-preferences on the results for presentation-rates, precaution (ii) allowed serial order intrusions (see below) to show themselves unambiguously. The lists themselves were originated by a random procedure but re-arranged by the experimenter so as to fit the conditions just mentioned. A typical list was LVMKTNCS.

Ideally the four groups of presentation-rates should have been presented in all possible permutations within the rolls, but it was felt that this would over-complicate this preliminary experiment. Two types of rolls were in fact made, on one of which the order of groups ran 1—1·33—2—4 letters per sec. (slow-to-fast rolls abbreviated as "S—F"), on the other of which the groups ran in reverse order (fast-to-slow rolls, abbreviated as "F—S").

Presentation. Subjects sat at a table before a "tracking-machine" through which the rolls were run. This consisted of an arrangement of rollers driven by a variable speed electric motor, allowing a roll of paper to be run smoothly across them. A slit 1 cm. wide in the cover of the machine allowed one letter to be seen at a time, moving towards the subject. Each letter was just under a cm. high. When the machine was switched on, the subject saw first the warning-line, then, after about a sec., the first letter, and so on until the eighth letter: the finishing-line followed the eighth letter by about ½ sec. The machine-speed was adjusted at the start of the experiment and thereafter kept constant, differential letter-spacing providing for the variations in presentation-rate. At the fastest rate (4 letters per sec.) the second letter appeared into view just as the first letter disappeared but for the other conditions a blank interval intervened, varying in duration according to the rate.

Each roll was done as a whole, no warning being given when, after every fourth list, the rate changed to a faster or slower one, depending on whether an S—F or an F—S roll was in the machine. The whole of each roll was devoted to one of the R, M, W, V conditions with either "said" or "written" recall throughout. Thus each subject did all possible conditions, working through eight rolls. In addition there was a practice-roll giving four lists at each rate but with the separate lists being done under different R, M, W, V and recall conditions. The eight rolls were worked through in a single session which varied from 1 hr. to 2½ hr. in length depending on the subject. The whole experiment had a balanced design as a precaution in case fatigue or practice effects occurred. The second half of each session consisted of the R, M, W, and V conditions presented in the same order as in the first half of the session but with different recall methods and different rolls. The eight rolls used were also balanced across the conditions (except the presentation-rates) to minimize any effects of accidental differences in the difficulty of the material.

Recall. "Written" recall was given on slips of paper on which was duplicated a grid with eight squares in each of which was to be placed the appropriate letter. These slips were rested upon the machine just below the viewing-slit so that a minimal delay occurred between the end of the list and the start of recall. The "said" recall was recorded on tape via a microphone placed on the table at the subject's right hand. When the experimenter saw the finishing-line appear (via a mirror), he switched the machine off and the subject began his recall. At the same time as switching off the machine, the experimenter switched on the tape-recorder. These actions gave rise to a single "click" which was a constant feature of the experiment. One subject reported it to be distracting; most of them used it as an additional signal to recall, both for "said" and "written" recall.

Scoring of results

The lists were marked as follows:

(1) The main index of correct recall was the number of "hits," i.e. the number of letters given in their correct positions in the list.

(2) The following error-types were scored.

(a) *Transpositions* (abbreviated as Ts). *A letter was considered transposed if it occurred in the recalled list in a position other than that it occupied in the presented list.*

Transpositions usually occurred in sequences of two or three letters occupying each other's positions, e.g. "XC" instead of "CX," "CXB" instead of "BCX." If "BCX" had been recalled as "XCB" this would have signified two letters transposed. Transposed sequences could of course involve any number of letters up to eight, though a transposed sequence of such length was very rare.

(b) *Acoustic confusions* (ACs). *An acoustic confusion was considered to have occurred if one letter from any of the groups* (*BCDPTV, FLMNSXZ, HJK*) *was replaced by another letter from the same group.*

The choice of letters in each of the groups above was decided by the fact that the letters in each single group have common vowel components ("ee," "e" (as in "yet"), "ay," respectively). Conrad (1962) has shown experimentally that the letters BCPTV are more likely to confuse with each other than with any of the letters FMNSX and vice versa, this being true of both a recall-test and a test of acoustic confusion through noise.

(c) *Serial order intrusions* (SOIs). *A letter was counted as a serial order intrusion if, when it appeared at the x-th position of a given recall of a list, it duplicated the letter at the x-th position of the list given as the immediately preceding recall.*

Note: the lists were so devised that such a duplication must necessarily be an error. The only exception to this rule was the (rare) accident of a correctly recalled letter being the same as a wrongly recalled letter in the same position of the previous list. This duplication would obviously not count as an SOI.

(d) In addition to the above classifications three minor classifications were noted, which taken together accounted for only about four per cent of the total errors. (i) Sometimes one or more of the transposed letters was apparently replaced by an acoustic confusion (e.g. "XV" instead of "BX"), or by an omission (e.g. "X–C" instead of "CBX"). In the main analysis it was decided to treat such omitted or acoustically confused letters as omissions or as acoustic confusions, rather than as transpositions. Their proportions, however, are shown separately in Table V. (ii) Sometimes two *neighbouring* letters from the same acoustic group were transposed, and one of them also confused (e.g. "VB" instead of "BP"). Such occurrences have been counted as one letter wrong, and categorized under "A^c." Although forming only 0·8 per cent. of the absolute total number of errors, they are shown separately in the main error-analysis.

(e) *Remainder errors* (Rs). *All recalled letters which were wrong, but could not be assigned to one of the preceding classifications, were called "remainder errors"* (*e.g. "C" instead of "X"*).

(f) *Omissions. Whenever the subject wrote or said "blank," this was counted as an omission.*

RESULTS

About 57 per cent. correct recall was obtained, averaging over all conditions. The difference between totals on S—F and F—S rolls was not significant (χ^2 round median, 0·39). The results from the two kinds of rolls were therefore pooled. There was no significant difference between the totals for each of the eight rolls (i.e. they did not vary significantly in apparent difficulty). The number of letters correct per list was normally distributed, taking all results together.

Table I shows mean scores for the various conditions. We may examine these scores in more detail.

(i) *RMWV*. Jonckheere (1954) has provided a test for assessing the significance of order-relationships in a series. It may be shown that scores increase with statistically significant regularity as we move along the RMWV dimension. Applying the test to the "say" matrix of Table I we have $S = 62$ ($p < 0{\cdot}001$), to the "write" matrix $S = 60$ ($p < 0{\cdot}002$), to the combined results $S = 74$ ($p < 0{\cdot}0001$).

t-tests on the average differences between vocalization-conditions are given in Table II, which also gives *t*-values for the average differences between "adjacent" rate-conditions.

TABLE I

MEAN NUMBER OF LETTERS RECALLED FOR GROUPS OF FOUR LISTS

(Maximum = 32)

	Say					*Write*				
	R	*M*	*W*	*V*	*Mean*	*R*	*M*	*W*	*V*	*Mean*
Rate: 1 ..	17·5	16·9	16·8	18·6	17·2	20·8	19·7	21·5	23·6	21·5
1·33 ..	14·4	17·0	15·5	17·2	15·9	19·7	18·7	20·7	22·9	20·5
2 ..	15·4	14·8	15·3	17·6	15·8	18·5	18·6	19·7	22·8	19·9
4 ..	12·7	14·7	16·9	17·2	15·2	17·1	18·6	19·0	20·3	18·8
Mean ..	15·0	15·85	16·1	17·65	16·15	19·0	18·9	20·2	22·4	20·1

	Combined				
	R	*M*	*W*	*V*	*Mean*
Rate: 1 ..	19·1	17·8	19·1	21·1	19·3
1·33 ..	17·0	17·2	18·1	20·0	18·1
2 ..	16·9	16·7	17·5	20·2	17·8
4 ..	14·9	16·6	17·4	18·7	17·0
Mean ..	16·95	17·1	18·0	20·0	18·0

(ii) *Rates.* Jonckheere's test applied to the rates gave S-values of 40 ($p < 0{\cdot}03$). 46 ($p < 0{\cdot}01$) and 42 ($p < 0{\cdot}02$) for "said," "written" and combined lists respectively, If the amount recalled is plotted against log (letters per sec.) a decreasing roughly linear relationship is obtained, though it was not attempted to fit a curve to the data. However, on inspection the data appeared to be consistent with the logarithmic hypothesis mentioned in the Introduction, the slowest rate appearing to vary most from the rule.

TABLE II

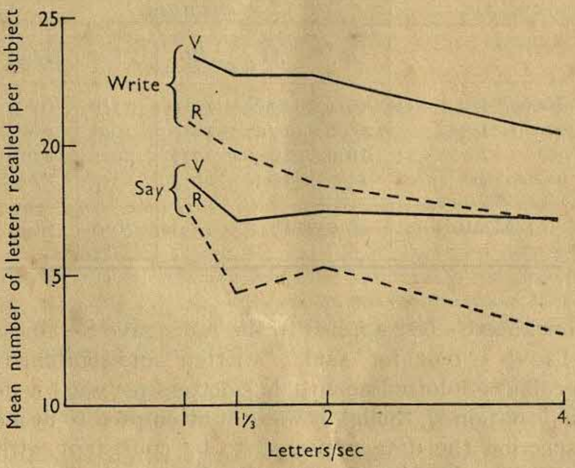
RESULTS OF *t*-TESTS ON COMPARISONS BETWEEN (A) VOCALIZATION-CONDITIONS AND (B) PRESENTATION-RATES

	(*A*)			(*B*)		
	R–M	*M–W*	*W–V*	1–1·33	1·33–2	2–4 (*letters per sec.*)
Say	0·34	0·72	2·53**	3·08**	—	0·79
Write	—	2·03	3·46***	2·35*	1·03	2·12*
Combined ..	0·62	2·09*	3·22***	3·65**	0·69	1·6
	*p <·025 **p <·01 ***p <·005 (all one-tailed)			*p <·05 **p <·01 (all two-tailed)		

(iii) *Say–write.* The difference between mean totals of "said" and "written" recall gave $t = 11{\cdot}05$ ($p < 0{\cdot}001$, two-tailed), in favour of "written."

(iv) *Interactions.* The most interesting interactions appear to be between the degree of vocalization (R, M, W or V) and the presentation-rates. Figure 1 isolates the V and R scores for clarity. As a measure of the advantage of V over R, the score for R-fast was subtracted from that for V-fast, for each subject. Similarly the score for R-slow was subtracted from that for V-slow. The resulting two groups of differences were then compared by a signs test. For "written" recall, $p < 0{\cdot}27$; for "said," $p < 0{\cdot}07$; for the combined scores, $p < 0{\cdot}01$. That is, the superiority of V over R was significantly greater at fast rates than it was at slow rates (using this measure), if the results were combined: though for "said" and "written" taken singly the advantages failed to reach statistical significance. The combined result, at least, is consistent with von Sybel's conclusion (1909).

FIGURE 1



(v) *Error-analysis.* Of the letters presented, 57·125 per cent. were correctly reported, 11·25 per cent. were omitted and 31·625 per cent. were incorrectly reported. As omissions were allowed we might expect these to be affected not only by limitations of memory but also by subjective factors; for example, one subject regularly adopted the policy of not attempting to recall more than four or five of a list. If we rate subjects according to their numbers of hits, Quartile I containing the highest scorers, we have the results shown in Table III. The "incorrect" percentages seem to vary

TABLE III

PERCENTAGE CORRECT, OMITTED AND INCORRECT FOR RANKED SUBJECTS
(Quartile 1 highest scorers)

Quartile	*Percentage correct*	*Percentage omitted*	*Percentage incorrect*
1	69·2	6·4	24·4
2	59·2	13·2	27·6
3	55·1	10·3	34·6
4	45·0	15·1	39·9

more consistently with changes in the "correct" percentages than do the omission percentages. The errors overall are divided as in Table IV. It will be seen that transpositions account for a larger proportion of errors than do omissions. These features have led us to score the errors in terms of their percentage proportion of total number of letters unsuccessfully attempted (i.e. percentage proportion of incorrect minus omissions). These scores are shown in Table V.

TABLE IV

DISTRIBUTION OF ERRORS OVER THE WHOLE EXPERIMENT

	Omissions	*OT**	*Trans-positions*	*AT***	*AC*	*A^c****	*SOI*	*Remainder*
Absolute frequency ..	2592	276	3251	93	1648	87	823	2159
Percentage of total errors	24·5	2·72	30·7	0·88	15·7	0·8	7·8	20·5

* Omissions in transposed sequences.
** Acoustic confusions in transposed sequences.
*** Errors of the type "VB" instead of "BP."

TABLE V

DISTRIBUTIONS OF ERRORS OVER ALL CONDITIONS

(Errors expressed as percentages of letters unsuccessfully attempted.)
(Cross-group SOI's are excluded.)

	Say															
	R				*M*				*W*				*V*			
	1	1·33	2	4	1	1·33	2	4	1	1·33	2	4	1	1·33	2	4
T	34·4	41·7	30·0	40·5	40·1	31·8	40·6	38·8	33·1	29·8	32·8	31·4	39·0	46·1	43·2	36·9
AC	16·4	15·5	20·1	14·1	20·6	25·5	21·6	19·7	26·4	22·9	22·6	25·1	25·4	21·7	25·5	15·5
SOI	8·8	8·6	7·2	9·8	7·1	8·2	7·6	7·0	10·6	5·1	8·7	6·9	12·7	8·6	6·9	6·8
R	38·6	33·2	41·4	35·3	31·9	33·8	29·6	34·5	29·2	41·1	35·2	34·5	21·9	22·1	23·6	40·4
A^c	1·7	1·0	1·3	—	0·4	0·7	0·6	1·7	0·7	1·1	0·7	2·1	1·0	1·5	0·8	0·4

	Write															
	R				*M*				*W*				*V*			
	1	1·33	2	4	1	1·33	2	4	1	1·33	2	4	1	1·33	2	4
T	24·4	26·7	20·6	35·6	48·9	43·9	36·1	44·4	43·6	39·0	36·7	33·3	36·4	33·2	40·1	40·2
AC	18·9	17·7	21·7	16·5	16·9	17·6	21·4	20·9	17·6	27·5	22·8	19·4	26·8	21·7	24·3	20·3
SOI	7·2	11·5	11·0	8·8	13·2	6·3	10·7	10·8	7·3	7·3	8·0	9·7	5·9	7·4	9·0	6·6
R	48·0	42·5	45·1	38·2	21·0	30·9	31·9	22·2	30·0	24·0	30·8	37·6	29·7	36·6	23·8	31·1
A^c	1·5	1·6	1·6	0·9	—	1·3	0·6	1·7	1·5	2·2	1·7	—	1·2	1·1	2·8	1·8

The main conclusions we may draw from Table V are as follows:

(*a*) *Errors as a function of presentation-rate.* Rate-differences led to few differences in the overall proportion of transpositions, SOIs, or ACs.

(*b*) *Errors as a function of vocalization.* ACs increased as we moved R—V, Jonckheere's tests giving S-values of 48 ($p < 0·01$), 44 ($p < 0·02$) and 64 ($p < 0·001$)

for said, written and combined lists respectively. On "say" Ts appeared on inspection to be more frequent for V than for W; on "write," Ts appeared on inspection to be more frequent for M than for R; neither difference reached statistical significance however. SOIs remained roughly constant over RMWV, and between "said" and "written" recall.

CONCLUSION AND DISCUSSION

The main findings may be summarized as follows:

(1) Combining the results over rates, V gave better recall than W, and W than M; M and R differed insignificantly.

(2) The presentation-rate of 1 letter per sec. gave better recall than 1·33 or 2 letters per sec.: the difference between 2 and 4 letters per sec. was most marked for written recall.

(3) Written recall was better than said recall.

(4) Recall was monotonically associated with RMWV most clearly for the fast rates; the slower the rate, the less the tendency showed itself, specially at the "R" end (for both "say" and "write").

(5) On comparing the difference between recall for "slow" and recall for "fast" lists, this difference was less if lists were vocalized during presentation, than if they were observed silently: but though this was significantly true overall, it was not significantly true for "said" or "written" reproduction taken individually.

(6) Correcting to allow for omissions, the following were the main error-trends.

(*a*) ACs increased significantly with increasing vocalization.

(*b*) Ts did not change significantly as a function of presentation-conditions: apparent differences associated with variations in recall conditions were also non-significant.

(*c*) The different types of errors (though see (*b*) above) were distributed about equally between "said" and "written" recall.

Before discussing the main issues arising from the present work, we may note that at least two previous findings were repeated: (*a*) there seemed to be a relationship between the number of letters perceived per unit time and the number recalled (though the present work was not specifically designed to test the logarithmic hypothesis mentioned in the Introduction); and (*b*) vocalizing the letters at presentation led to their being better recalled (see Introduction).

We may now single out some items for discussion.

(i) Result (4) above suggests that when a vocalization-effect shows itself, it is most clearly shown as monotonic at fast rates: the slower the rate in this experiment, the less a similar monotonic effect was apparent. Inspection suggests this to be because slow rates seemed to give some advantage for recall for condition R. This gives rise to a U-curve when recall was plotted against amount of vocalization (for slow rates). A possible explanation of this result might be that, at slow rates perceived silently, the facilitatory effect of vocalizing is relatively diminished in comparison with the advantages resulting from intra-list rehearsal (which we might surmise particularly to affect lists perceived silently). Introspections obtained from subjects showed that in this experiment nearly all subjects did rehearse during presentation of the slow lists. This still leaves open the question of why vocalizing should be superior. A number of possible explanations may be offered: a subject attends more when vocalizing; he uses more musculature; he is more able to rhythmicise (cf. McGeoch, 1942); the auditory store might in some way be more efficient than the visual (as Buschke has suggested, 1962); the subject obtains more information because he hears the letters in addition to seeing them. Detailed discussion of these possibilities is outside the scope of this paper.

(ii) Result (5) above suggested that when vocalizing led to better recall, its advantages were relatively more marked for fast lists than for slow lists, though this was not statistically significant for "said" or "written" recall individually. This finding should be distinguished from that of (i) above: it would have been possible to have numerical values in Table I, which did not lead to the conclusion that vocalization has relatively more advantage for fast lists as opposed to slow lists, despite being monotonically associated with recall more clearly for fast lists.

It was noted in the Introduction that from Broadbent's theory the opposite result might have been predicted on the grounds that the increased sensory input of vocalizing at fast rates would have overloaded the serially limited single channel. This result might therefore be considered as damaging to the theory. However, the author feels that it need not be so, if account is taken of two other possibilities. (*a*) The above prediction assumes that the channel passes a single letter, stores it, then deals with the next letter and so on. But we do not know whether the channel is necessarily restricted to dealing with one letter as its "basic unit": it might be possible for the vocal input to be held in store until after the passage of the complete visual input. This would only be likely if the vocal store had a high storage capacity. It is also possible that the visual information is coded into vocal information so rapidly that only vocal information is stored, the visual information decaying while the vocal is being dealt with in the single channel. In either case, the number of inter-modality "switches" per list would be considerably less than the number of items per list: and if so, would throw doubt on the assumption that a vocalized input at fast rates need necessarily overload the system. (*b*) Hitherto little attention has been paid to the question of whether arousal can affect storage-processes. But it does not seem unreasonable to suppose that it might: and if the increased afferent input associated with voicing served also to increase arousal (e.g. by midbrain or thalamic reticular formation stimulation), it might be that this in turn facilitated the storage-process. Welford (1962) has suggested that arousal might influence decision-processes: our analogous hypothesis suggests that memorizing would also be facilitated, possibly by increasing the transmission rate or information-handling rate in the hypothetical single channel. But, as Hebb (1955) emphasized, we need more experimentation on the relation between arousal-level and optimal learning efficiency.

(iii) In the Introduction, the error-analysis was said to have been undertaken in the hope of finding out whether any error-types seemed to be particularly affected by presentation- as opposed to recall-conditions. The results showed that the AC proportions increased with vocalization. But their proportions were approximately equal for both "said" and "written" recall. This suggests that ACs were more likely to arise during or just after presentation (i.e. in store), rather than in recall. Transpositions, on the other hand, gave a rather anomalous result insofar as inspection showed their distributions to vary partly with vocalization and partly with recall-method: these differences however failed to reach statistical significance. Hence there is a suggestion, but no more, that the transpositions may have arisen in the course of recall, in addition to their possibly having confused in store. Finally, SOIs did not appear to vary consistently with either presentation- or recall-conditions. These features of SOIs suggest that their occurrence might have been due partly at least, to subjective factors (e.g. surprise or distraction) rather than to actual changes in store: possibly a subject resorts to repeating items from previous recalls when he is disturbed in recalling a present item. Our finding is consistent with Conrad's finding (1960) that SOIs in digit lists did not vary regularly with other measures of forgetting (e.g. omissions).

These results concerning various errors need much more experimental support before they can be generalized. The fact that ACs varied with presentation-conditions, but not recall-methods, is of interest in view of Conrad's suggestion (1964) that ACs may be examples of literally fading traces. However, it should be noted that ACs *increased* with vocalization, which otherwise led to fewer errors: commonsense suggests this to have been due to the subject's relying on cues derived from hearing his own voice. In future work we hope to elaborate on this.

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SHORTER ARTICLES AND NOTES

SECONDARY REINFORCEMENT VALUE OF STIMULI ASSOCIATED WITH SHOCK REDUCTION

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An experiment is described in which 32 rats were tested to determine if an escape box which had been previously associated with shock reduction had secondary reinforcement value. Subjects were tested in both a fear and a neutral situation. Significant secondary reinforcement effects were found in both situations. However, contary to expectations, greater secondary reinforcement effects were obtained in the neutral than in the fear situation.

INTRODUCTION

Mowrer (1960) has pointed out that with regard to the use of shock for the study of secondary reinforcement, the results have been highly inconsistent. In order to illustrate this point he cited ten studies, all of which purport to test the hypothesis that stimuli associated with shock reduction will become secondary reinforcers. A review of the studies cited by Mowrer revealed that only the study of Smith and Buchanan (1954) and that of Goodson and Brownstein (1955) obtained positive results. Mowrer then stated that the reason these studies have not obtained consistent positive results lies in the absence of the fear drive in the trials designed to test the efficacy of the secondary reinforcing stimulus. Typical of the studies which have tested subjects in neutral situations and obtained negative results is that of Littman and Wade (1955). However, since no previous studies have compared subjects tested for secondary reinforcement effects in fear and neutral situations, there is no directly relevant evidence to support Mowrer's view. There exists indirect support for Mowrer's view in the evidence on secondary reinforcers based upon the drives of hunger and thirst. As Miller (1951) has pointed out, secondary reinforcement effects fail to appear when the primary drive of hunger or thirst is absent. The purpose of the present study, therefore, is to test the proposition that the presence of fear during the test trials will enhance the secondary reinforcer efficiency of stimuli associated with fear reduction.

METHOD

Subjects. Thirty-two male rats, Tryon S1 strain, all 130 days old, were used in the experiment. They were previously inexperienced and were handled for ten minutes a day for a week prior to the beginning of the training trials. All subjects were provided with *ad lib.* food and water in their home cages. They were kept in group cages with three subjects in each cage and housed in the experimental room which was maintained at a temperature of 72° F.

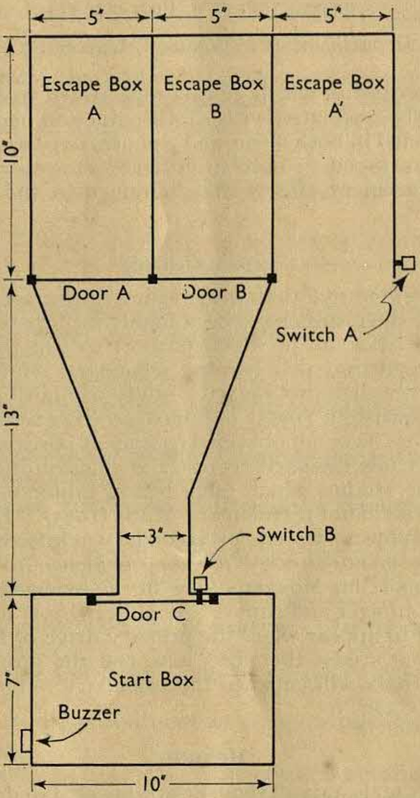
Apparatus. Figure 1 shows the apparatus used in the present experiment for training the subjects and testing them in a fear situation. The start box was grey with a transparent top and a grid floor. It was eight inches deep and the floor was constructed of 3/32 in. stainless steel rods placed ½ in. apart. A guillotine type door (C) connecte d it with a Grice box type alley way. This was similar in height, colour, and flooring to t hestarting box. A switch (B) was placed so as to be thrown by the opening of the door. This switch activated the shock in the grid floor in both these compartments, and started a buzzer which sounded for 0·5 sec. It also started an electric timer.

In order to create a neutral situation, the start box and the alley were altered. The grid floor was covered with a solid piece of plywood and the walls were covered with ⅛ in. aluminum sheeting. The buzzer was also disconnected.

Doors A and B were also of the guillotine type. Placed immediately behind these two doors were three escape boxes that were free to slide. Two of these boxes (A and A_1) were identical, with vertical black and white stripes of about ½ in. covering the inside.

These two identical escape boxes were employed in order to make it easier to change the positions of the two different types of escape boxes employed in the study. The third box (B) was solid black in colour. All the boxes were 8 in. high and covered with a transparent, hinged glass lid. The floors of all the boxes were a solid sheet of aluminum and were painted to match the rest of the escape box. The floor was hinged at the back so that the weight of the rat on the front would throw switch A, which turned off the electric clock.

FIGURE 1



The shocking current was provided by a variable transformer, a 110–750 v. step-up transformer, and a 700,000 ohm resistance in series with the animal. The resultant shocking current was approximately 0·9 ma.

Procedure. All subjects were placed in the apparatus in groups of four for 10 min. on two successive days. The following day they were placed in the apparatus individually for 10 min. During these trials all doors were open and the subjects were free to explore the entire apparatus. The subjects were then randomly divided into four groups of eight subjects each by means of a table of random numbers.

Two experimental groups were run. The training trials for these groups were identical. The subjects in both groups were divided into two sub-groups with four subjects in each. They were placed in the starting box individually and the door was opened. The shock buzzer and clock were thereby started. One sub-group of four subjects found only the door to the black escape box open and the other sub-group found only the door to the striped escape box open. The position of the boxes was alternated randomly. When an animal entered an escape box, the door was shut and he remained there for 30 sec. This procedure was repeated for 20 trials with a 60-sec. delay between trials.

Twenty-four hours after the completion of the training trials, the subjects in both experimental groups were given their test trials. Experimental Group N was tested in the apparatus as it was modified to create a neutral situation. Experimental Group F was tested in the original apparatus with the buzzer present, but without shock present. The subjects in both groups were placed in the start box and the door opened. The doors to both the escape boxes were open. The door to whichever escape box the subject entered was closed and the subject remained there for 30 sec. If the subjects refused to enter an escape box after 150 sec., the trial was terminated. As in the training trials the position of the escape boxes was alternated randomly. The subjects were given 20 trials with a 60-sec. interval between trials. The response time and escape box chosen were recorded for each subject.

Each subject was given a score that was based upon those trials upon which he entered an escape box. For those choice trials the percentage of the trials upon which the subject entered the training escape box was computed and constituted the subject's score. This score was employed since it eliminates from consideration the no-choice trials which are difficult to interpret with regard to secondary reinforcement effects. In addition to being compared on this measure of the relative frequency of entry into the training escape box the groups were compared on the mean number of entries into the training escape box.

Two control groups were run that were designed to determine the number of times a subject would enter each escape box when the boxes had not been associated with fear reduction. Thus Control Group N received treatment identical to Experimental Group N with the exception that during the training trials there was no shock present. Control Group F received treatment identical to Experimental Group F with the exception that during the training trials there was no shock present.

RESULTS

Comparison of Control Group N and Experimental Group N

Table I and Table II present the results for Control Group N and Experimental Group N. These two groups were tested with the neutral start box and alley. Table I reveals that Control Group N made 28 responses to the escape box which was present during the training trials and 42 responses to the new escape box. The mean score for Control Group N was 29·6, and the mean number of entries into the training escape box was 3·5. Table II shows that Experimental Group N made 122 responses to the training escape box and 38 responses to the new escape box. The mean score for Experimental Group N was 76·3 and the mean number of entries into the training escape box was 15·3. When the mean scores of the two groups were compared, the mean score of Experimental Group N was significantly greater at the 0·01 level of confidence ($t = 5{\cdot}99$). Experimental Group N also had a significantly greater mean number of entries into the training escape box (0·01 level of confidence; $t = 7{\cdot}47$). Thus significant secondary reinforcement effects were found when the subjects were tested in the neutral situation. The median response time for all subjects in Experimental Group N during the training trial was 0·9 sec. and during the test trials it was 5·9 sec., indicating that the neutral condition was markedly different from the training conditions.

TABLE I

ESCAPE BOXES CHOSEN BY CONTROL GROUP N DURING THE TEST TRIALS

Subjects	*Number of choices of training escape box*	*Number of choices of new escape box*	*Number of no choice trials*	*Score*
1	0	1	19	0
2	2	3	15	40
3	10	10	0	50
4	0	2	18	0
5	2	3	15	40
6	4	10	6	29
7	1	2	17	33
8	9	11	0	45
	$\Sigma = 28$	$\Sigma = 42$	$\Sigma = 90$	$\bar{X} = 29{\cdot}6$

TABLE II

ESCAPE BOXES CHOSEN BY EXPERIMENTAL GROUP N DURING THE TEST TRIALS

Subjects	*Number of choices of training escape box*	*Number of choices of new escape box*	*Number of no choice trials*	*Score*
1	15	5	0	75
2	14	6	0	70
3	14	6	0	70
4	16	4	0	80
5	12	8	0	60
6	17	3	0	85
7	15	5	0	75
8	19	1	0	95
	$\Sigma = 122$	$\Sigma = 38$	$\Sigma = 0$	$\overline{X} = 76{\cdot}3$

Comparison of Control Group F and Experimental Group F

Table III and Table IV present the results for Control Group F and Experimental Group F. These two groups were tested with the training start box and alley. Table III reveals that Control Group F made 43 responses to the escape box which was present during the training trials and 86 responses to the new escape box. The mean score for Control Group F was 38·3 and the mean number of entries into the training escape box was 5·4. Table IV reveals that Experimental Group F made 95 responses to the training escape box and 65 responses to the new escape box. The mean score for Experimental Group F was 59·4 and the mean number of entries into the training escape box was 11·9.

TABLE III

ESCAPE BOXES CHOSEN BY CONTROL GROUP F DURING THE TEST TRIALS

Subjects	*Number of choices of training escape box*	*Number of choices of new escape box*	*Number of no choice trials*	*Score*
1	4	3	13	57
2	4	15	1	21
3	2	18	0	10
4	5	3	12	63
5	9	6	5	60
6	8	12	0	40
7	2	18	0	10
8	9	11	0	45
	$\Sigma = 43$	$\Sigma = 86$	$\Sigma = 31$	$\overline{X} = 38{\cdot}3$

When the mean scores of the two groups were compared, the mean score of Experimental Group F was significantly greater at the 0·05 level of confidence ($t = 2{\cdot}29$). Experimental Group F also had a significantly greater mean number of entries into the training escape box (0·01 level of confidence; $t = 4{\cdot}71$). Thus significant secondary reward effects were found when the subjects were tested in the fear situation. The median response time for all subjects in Experimental Group F during the training trials was 0·9 sec. and during the test trials it was also 0·9 sec., indicating that a strong fear drive was present during the test trials.

TABLE IV

ESCAPE BOXES CHOSEN BY EXPERIMENTAL GROUP F DURING THE TEST TRIALS

Subjects	*Number of choices of training escape box*	*Number of choices of new escape box*	*Number of no choice trials*	*Score*
1	15	5	0	75
2	10	10	0	50
3	10	10	0	50
4	10	10	0	50
5	13	7	0	65
6	10	10	0	50
7	10	10	0	50
8	17	3	0	85
	$\Sigma = 95$	$\Sigma = 65$	$\Sigma = 0$	$\bar{X} = 59{\cdot}4$

Comparison of Experimental Group F and Experimental Group N

Table II presents the results for Experimental Group N and Table IV presents the results for Experimental Group F. Experimental Group F was tested with the fear condition present and Experimental Group N was tested with the neutral conditions present. The mean score for Experimental Group N was significantly greater at the 0·02 level of confidence ($t = 2{\cdot}74$) than the mean score for Experimental Group F. Experimental Group N also had a significantly greater mean number of entries into the training escape box (0·01 level of confidence; $t = 2{\cdot}74$). Thus greater secondary reinforcement effects were found for subjects tested in the neutral situation than for those tested in the fear situation.

DISCUSSION

The results of the comparison between Control Group N and Experimental Group N support the earlier findings of Smith and Buchanan (1954), and Goodson and Brownstein (1955) that the secondary reinforcement value of stimuli associated with shock can be demonstrated in a neutral situation. This finding raises the question of what is unique about the present study and those of Smith and Buchanan, and Goodson and Brownstein that enabled them to obtain significant results where other studies have failed. One possible explanation lies in the fact that these studies have tested for secondary reinforcement effects in an experimental situation which was not used in the other studies. In these studies the subjects were placed in a choice situation in which one alternative was a neutral escape box, and the other was an escape box that had previously been associated with shock reduction. Both boxes were visible at the choice point. Typical of the procedure used in the studies which have obtained negative results is that of Littman and Wade (1955). They tried to determine if a subject would learn a new response (e.g. bar pressing) in order to obtain a stimulus that was previously associated with shock reduction. This certainly is a quite different and more stringent test than that of simply approaching a visible box that was previously associated with shock reduction.

The results of the comparison between Control Group F and Experimental Group F indicate that secondary reinforcement effects can be demonstrated in the presence of the fear drive as predicted by Mowrer (1960). However, the results of the comparison between the two experimental groups is directly contrary to Mowrer's prediction. The results of this comparison clearly indicate that the neutral situation produced greater secondary reinforcement effects than did the fear situation. Since there is some evidence that a strong drive can cause a discrimination habit to break down, it is possible that had the subjects been tested with a weaker fear drive present the results may have been more favourable to Mowrer's proposition. Thus a future research project might compare the amount of secondary reinforcement effects found for subjects tested with different degrees of fear present.

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EFFECT OF EFFORT ON RESISTANCE TO EXTINCTION OF THE BAR-PRESSING RESPONSE

BY

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This study was designed to obtain more information on the relation between effort and resistance to extinction when all subjects receive equal acquisition experience on each effort level at which the response is subsequently to be extinguished. Twenty-four subjects were given 120 reinforcements on each of three weighted bars of 5, 40 and 70 gm. They were then ordered into three groups, each extinguished on one effort level. Analysis of the number of responses emitted in six daily 20-min. extinction sessions showed that resistance to extinction was not related to the amount of effort expended in extinction.

Experiments that test the hypothesis that resistance to extinction is a function of the effortfulness of the response have produced equivocal results. Early studies supporting the hypothesis (e.g. Mowrer and Jones, 1943; Solomon, 1948; Applezweig, 1951) have been criticized because habit strength was not equated for all groups before extinction began. Consequently, the reported superior resistance to extinction of low effort groups might just as well be due to greater habit strength.

Accordingly, more recent experiments have attempted to control habit strength prior to extinction but have produced conflicting results. Maatsch, Adelman and Denny (1954) using effort levels of 5, 40 and 80 gm. in a bar-pressing situation found no significant differences in the number of responses emitted in a 60-min. extinction session. Aiken (1957) likewise failed to find significant differences in resistance to extinction between high and low effort groups using a panel pressing apparatus. However, Capehart, Viney and Hulicka (1958) report a study using effort levels of 5, 40 and 70 gm. in a Skinner Box in which number of responses emitted in extinction varied inversely with effortfulness of the response.

It is clear that further research is still necessary to assess the validity of the assumption that high effort responses extinguish more quickly than low effort ones. The present experiment was therefore designed to obtain more information on the relation between effort and resistance to extinction in the bar-pressing situation. The design follows that used by Capehart *et al.*, in which all subjects receive equal acquisition experience on each effort level at which the response is subsequently to be extinguished. Two additional features are included in the present design. As it was suggested (Capehart *et al.*) as a possible limitation of their experiment that further extinction sessions should have been observed, six daily 20-min. extinction sessions were included in the present design. Further, as previous experiments make no mention of equating groups for body weight, and since this may be a variable partly responsible for the equivocal results to date, body weights of effort groups were equated in the present experiment.

METHOD

Subjects

Subjects were 24 naïve hooded rats from the Wistar Strain maintained by the University of Otago Medical School, New Zealand. At the beginning of the experiment ages ranged from 200 to 300 days with a mean age of 280 days. Mean body weight was 220·5 gm. with a range from 219·5 to 222·0. They were maintained on 10 gm. of food a day supplemented by greens once a week. Water was always available in the home cages. The usual handling and taming procedures were followed. At the beginning of each experimental session subjects had been deprived of food for approximately 23 hr.

Apparatus

The apparatus was a Skinner Box with a bar requiring a minimum effort of 5 gm. to move it through a distance of 1¼ in. to activate the food dispensing mechanism. Counterweights of 35 and 65 gm. could be attached to the back of the bar to provide three effort levels of 5, 40 and 70 gm. Responses were recorded on an electric counter. The food reward was a pellet of spaghetti 0·025 gm. in weight.

Procedure

Preliminary training. All subjects were placed in the apparatus for 10 min. a day and allowed to eat 10 pellets from the food tray. This lasted for 5 days. Beginning on day six, the experimenter manually activated the feeding mechanism which produced a click and delivered one pellet into the food tray. This was to enable the rat to associate the click with the arrival of a pellet in the food tray. Each rat was given 10 pellets a day for 5 days. The bar was never inside the apparatus during preliminary training.

Acquisition. Immediately following preliminary training acquisition training was commenced and lasted 12 days. On day one, all subjects were required to make 30 reinforced responses on the 5 gm. bar; on day two, 30 responses on the 40 gm. bar, and on day three, 30 responses on the 70 gm. bar. For the next nine days the bar loadings by day were: 5, 40, 70, 5, 40, 70, 5, 40, and 70. At the completion of acquisition training therefore, all subjects had made a total of 360 reinforced responses, 120 at each effort level.

Extinction. At the conclusion of acquisition training subjects were ordered into three groups (N = 8) equated for body weight and response rate on the last six days of acquisition. Group 1 (mean weight = 219·75 gm.) was extinguished on the 5 gm. bar, Group 2 (mean weight = 220·00 gm.) on the 40 gm. bar and Group 3 (mean weight = 220·50 gm.) on the 70 gm. bar. Extinction was measured in terms of the number of responses made on six daily 20-min. sessions. Responses were not rewarded by food during extinction.

RESULTS AND DISCUSSION

Analysis of variance of mean bar presses by each effort group in six twenty-minute extinction sessions was carried out. The F for effort levels was not significant (F = 0·78, *d.f.* = 2/21) indicating that effort expended in extinction was not related to the number of responses emitted. The interaction between effort levels and extinction sessions was also not significant (F = 1·27, *d.f.* = 10/105). This indicates that relations between the three effort groups did not change significantly over the six extinction sessions. The only significant source of variation was extinction sessions (F = 10·98, *d.f.* = 5/105, $p < 0{\cdot}01$) which indicates that the mean number of responses emitted by all effort groups declined comparably with successive extinction sessions.

The results of this experiment therefore clearly fail to confirm the recent finding of Capehart, Viney and Hulicks (1958), who showed that the number of responses in extinction varied inversely with amount of effort. No explanation for the opposed findings is readily apparent. The present study used the same design as Capehart *et al.*, the only procedural difference being the number of reinforcements in acquisition. In the Capehart *et al.* study, subjects received a total of 90 reinforcements, 30 at each effort level while in the present experiment 360 reinforcements were administered, 120 at each effort level. It appears unlikely, however, that this would be a significant variable.

On the other hand the present experiment confirms the results obtained by Maatsch, Adelman and Denny (1954) and suggests that within the effort levels used in the typical bar-pressing experiments, equating habit strength in acquisition serves to eliminate effort as a significant source of variation in extinction.

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THE INFLUENCE OF STIMULUS SCALE-INTERVAL UPON THE ASSESSMENT OF PAIN THRESHOLD

BY

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An experiment concerning the influence of the scale of stimulus values upon the perception of heat-pain is reported in which it is found that the value of the threshold stimulus is dependent upon the size of the steps between successive stimuli.

The results are analysed in the light of a paper by Brown and Cane (1959) in which they point out that the value of a sensory threshold yielded by the Limiting Method is mathematically dependent upon the size of the steps between successive values of the variable stimulus. The threshold values reported here are found to be dependent on the step-size between stimuli to a greater extent than that which would be predicted by Brown and Cane.

In view of these results, an attempt is made to explain the wide variety of pain threshold values reported in the literature.

INTRODUCTION

Two papers have drawn attention to the fact that various reports of the threshold of pain produced by radiant energy applied to the forehead show a wide range of mean values, from 182 to 348 mc./sec./cm^2 (Clausen and King, 1950; Hardy, Wolff and Goodell, 1952). (It should be noted that the figure of 348 mc./sec./cm^2 refers to an experiment carried out by Schilling and Musser other than the one mentioned by Hardy *et al.* in their bibliography). Since Hardy *et al.* are convinced, on other grounds, that the heat-pain threshold among men and women is uniform, they suggest that these differences depend upon what is selected as the "end-point of pain." The term "end-point of pain" refers to the fairly well established finding that as a heat stimulus is increased in intensity the subject first experiences a "burning" sensation, then a "pricking" sensation and finally an "aching" sensation. Clausen and King (1950) attribute the lack of agreement to differences in the techniques employed by the various investigators, and in the calibration of their apparatus, and say that these factors must be brought under control if cross-comparisons of data are to be rendered useful.

An important point that appears to have been overlooked by investigators in this field, and not even mentioned in some reports, is the interval in energy level between successive stimuli. Accordingly, it was decided to carry out an experiment in which three different scales of stimulus values were used in the assessment of pain threshold, in order to examine their influence upon the threshold values obtained. The first scale progressed in steps of 32 mc./sec./cm^2, the second in steps of 16 mc./sec./cm.^{2} and the third in steps of 8 mc./sec./cm.^{2}.

METHOD

Subjects

Ninety-eight subjects were drawn from the academic and administrative staffs and the student body of the university. Group A consisted of 42 subjects (21 males and 21 females), Group B of 35 (19 males and 16 females) and Group C of 21 (11 males and 10 females).

Apparatus and procedure

A modified Hardy-Wolff (1940) radiant-heat apparatus was used, in which the heat from a 500-W. bulb was concentrated by a lens through an aperture 2 cm. in diameter. Mains power was supplied to the lamp through a Variac transformer and monitored by means of a voltmeter. The apparatus was calibrated by means of a Moll thermopile and millivoltmeter in terms of mc./sec./cm.^{2}, and the calibration was checked before each experimental session.

For the reasons mentioned by Chapman and Jones (1944, p. 82), a small area of skin in the centre of each subject's forehead was blackened with India ink. This area was exposed to the radiant-heat source for three seconds at one minute intervals. (Repeated stimulation of the same area of skin if done in rapid succession will cause lowering of the pain threshold and local hyperalgesia. However, Hardy, *et al.* (1952), have shown that with a one-minute interval between successive stimulation of the same skin area there is no lowering of the pain threshold.) The subject was told that the procedure was not an endurance test and was not intended to discover how much pain he could stand, but was an experiment to find out at what stage the stimulus was first felt as painful, that is, when he felt a small stab of pain at the end of the three-second exposure.

For all subjects, the energy level of the first stimulus was 98 mc./sec./cm.^{2}, which the majority of subjects described as warm. In the first group of subjects (Group A), the stimuli were successively increased by approximately 32 mc./sec./cm^2. In the second group (Group B), they were increased in steps of approximately 16 mc./sec./cm.^{2}, and in the third group (Group C) in steps of approximately 8 mc./sec./cm.^{2}. It was impossible to carry out the corresponding descending series because of the danger of blistering the skin at high intensities of stimulation.

Since Hardy, Goodell and Wolff (1951) reported skin temperature to have a modifying influence on pain threshold, forehead skin temperature was measured at the beginning of the experiment. Room temperature was always between 20·5° C. and 22° C.

RESULTS

Table I shows the mean and standard deviation of the pain threshold, the mean skin temperature and the mean age for each group of subjects.

TABLE I

MEAN PAIN THRESHOLD

Subjects	*Number of subjects*	*Mean age of subjects*	*Mean pain threshold mc./sec./cm.^{2}*	*Standard deviation*	*Mean skin temperature in ° C.*
Males	21	22·7	248	43·3	34·2
Females	21	21·1	228	23·1	34·15
Total	42	21·9	238 (230)	35·2	34·18

Group A (32 mc./sec./cm.^{2} intervals)

Males	19	22·0	238	22·4	33·85
Females	16	22·7	230	20·6	33·9
Total	35	22·4	234 (242)	21·6	33·88

Group B (16 mc./sec./cm.^{2} intervals)

Males	11	23·0	193	33·1	33·9
Females	10	20·2	166	41·7	33·85
Total	21	21·6	180 (202)	37·4	33·88

Group C (8 mc./sec./cm.^{2} intervals)

The figures in brackets refer to corrected means (see text).

A statistical analysis of the data was made by means of the Mann-Whitney U Test (Siegel, 1956). As can be seen from the Table, there is very little difference between the mean pain thresholds of Groups A and B. The mean pain threshold of Group C, however, is significantly lower than that of Group B ($p < 0{\cdot}00006$, two-tailed). There is no significant difference between pain thresholds for men and women in any group.

Assuming that the distribution of p is cumulative normal, the mean pain thresholds of Groups A, B and C were then adjusted by means of the formula given by Brown and Cane (1959, p. 122) from 238, 234 and 180 mc./sec./cm.^{2} respectively to 230, 242 and 202 mc./sec./cm.^{2} respectively. In spite of the correction factor, a t test indicates a significant difference between the means of Groups A and C, and also B and C ($p < 0{\cdot}05$). The difference between Groups A and B is just not significant at the 0·05 level of confidence (difference between the means = 12; standard error of the difference = 6·542).

This analysis indicates, therefore, that there is a residual effect of stimulus scale-interval even when correction has been made for the statistical effect predicted by Brown and Cane.

Discussion

It is the opinion of Hardy, Wolff and Goodell (1952) that pain threshold is uniform, and that the sensation of pain bears a highly predictable relation to the intensity of the stimulus. The results of the present experiment indicate, however, that the threshold for pain is to a great extent dependent upon the procedure adopted in its assessment.

Since in the present and previous experiments in which there were small energy intervals between stimuli (Clausen and King, 1950; Birren, Casperson and Botwinick, 1951) pain is reported at very low stimulus levels (approximately 182 mc./sec./cm.^{2}), the question arises as to which is the "correct" procedure and what is a "true" threshold?

It is interesting to note the following two experiments in the light of the present findings.

Birren *et al.* (1951) report a mean forehead pain threshold for 50 instructed subjects (that is, the sensations to be experienced were described to them) of 184·2 mc./sec./cm.^{2}. These authors used the same end-point as was used in the experiments of Hardy, Wolff and Goodell, and in the present study, namely the pricking pain threshold. Their results are approximately 50 mc./sec./cm.^{2} lower than the mean reported by Hardy, Wolff and Goodell; the stimulus intervals they used, however, were 10 mc./sec./cm.^{2}, as against 20 mc./sec./cm.^{2} in Hardy, Wolff and Goodell's experiment.

Clausen and King (1950) found a mean forehead pain threshold of 182 mc./sec./cm.^{2} for 31 instructed subjects, using the pricking pain threshold. They reported that stimulation began at 100 mc./sec./cm.^{2} and, generally speaking, the first two increases were fairly large (20 mc./sec./cm.^{2}) and thereafter steps of 10 mc./sec./cm.^{2} were used. They attribute their results to differences in calibration. However, the results of the present experiment indicate that they are probably due to the stimulus intervals.

It was thought that perhaps the pain threshold was significantly lower in Group C in the present experiment owing to the fact that many more stimuli were given and "sensitization" may have taken place. This possibility was checked by a further experiment on 18 student subjects using 32 mc./sec./cm.^{2} intervals and determining forehead pain threshold twice, consecutively. The threshold was unchanged the second time, despite the number of stimuli presented.

In the case of the high pain thresholds mentioned by Hardy *et al.* (1952), namely, those found by Chapman and Jones (1944) and Schilling and Musser (1948), the experimental conditions were somewhat different from usual. Chapman and Jones reported a mean pain threshold of 305 mc./sec./cm.^{2} in 200 subjects. The word "pain" however, was omitted from the instructions. Hall and Stride (1954) also found a significantly higher pain threshold in a psychiatric population when no mention of pain was made in the instructions. In the experiment carried out by Schilling and Musser (1948) the forehead and back of one hand were tested alternately, and a mean pain threshold of 348 mc./sec./cm.^{2} was reported. The subjects were instructed as to the endpoint of (pricking) pain, and the lowest level from either the forehead or the hand was taken as the threshold. It is felt that these two sets of results are not comparable with the others considered here.

In assessing pain threshold, it is not possible to adopt the usual psychophysical procedure of using both ascending and descending trials to obtain an accurate estimate of the threshold because of the danger of injuring the subject in the descending series. The size of the stimulus scale-interval is therefore critical, and threshold values reported by various investigators should not be compared unless the stimulus intervals and exact procedure are taken into account.

Because the information was required for another purpose, all the subjects who took part in the experiment reported here were assessed on the introversion/extraversion scale of the Maudsley Personality Inventory. As a matter of interest, comparisons were made between the influence of this personality variable upon pain threshold and the influence exerted by stimulus-scale interval. It was found that the difference in mean pain threshold as between Groups B and C (tested with 16 and 8 mc./sec./cm.^{2} stimulus intervals respectively) was greater than the difference between the mean threshold values for introverts and extraverts in Groups A and B. Also, whereas introverts were found to have a lower pain threshold than extraverts when tested with the two larger intervals (Groups A and B), the reverse was the case when measurement was made with the smallest stimulus interval (Group C). This seems to indicate that the purely procedural factor of choice of stimulus interval by the experimenter has a greater effect upon the pain threshold value arrived at than has the "psychological" variable of introversion/extraversion. Evidently there is good reason to look more closely, in the light of these results, at previous work on the relation between personality and attitudinal factors and the pain threshold.

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COUNTER DISPLACEMENT IN THE VISUAL FIGURAL AFTER-EFFECT

BY

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In this study an attempt is made to specify the conditions under which counter displacement (test contour displaced toward the inspection contour) will occur. Three experiments are described in which different lengths of inspection of the test- (T-) figure are given following short and long inspections of the inspection- (I-) figure. In Experiment I, after partial dissipation of the figural after-effect (FAE), a period of continuous inspection of the T-figure was employed. In Experiments II and III distributed inspection of the T-figure was given by increasing the number of measurements of the FAE. In all experiments counter displacement was evident. The results were consistent with an interpretation of counter displacement offered earlier by the authors.

INTRODUCTION

Investigations of the figural after-effect (FAE) reveal that displacement is one of the most readily observable features of the phenomenon. After prolonged fixation of an I-figure, a T-figure placed in the immediate area of the I-figure will appear to be displayed away from the original area of greatest satiation.

Köhler and Wallach (1944) and Osgood and Heyer (1952) have attempted to explain figural displacement and both explanations predict that displacement is normally away from the region of maximum satiation. However, there is evidence to indicate that under certain conditions a T-figure is displaced *towards* the area originally occupied by the I-figure. Such counter displacement, which has also beeen termed "negative" and "asymmetrical" displacement, has been reported by Smith (1954) and Sagara and Oyama (1957). More recently, the present investigators (Kelm, 1962; Kelm, Jensen and Ramsay, 1963; Prysiazniuk and Kelm, 1963) found consistent counter displacement in atypical subjects when single lines were used as I- and T-figures. Acute schizophrenics, alcoholics administered lysergic acid diethylamide, and mental defectives had a smaller FAE than normals immediately after inspection, and exhibited counter displacement 30 and 60 sec. after inspection.

An interpretation of counter displacement has already been proposed (Prysiazniuk and Kelm, 1963). The basic assumption, derived from some of Köhler's speculations, is that asymmetrical satiation develops around the T-line during measurements of the FAE. Development of this satiation is such that a reversal in levels of satiation about the T-line occurs, thus resulting in a reversal in the direction of displacement.

When an I-line is inspected for a prolonged period of time a symmetrical pattern of satiation will develop about that line. Satiation will be greatest at the I-line and will decrease with distance from the line. When the T-line is placed near the position of the I-line it will at first appear to be displaced away from the position originally occupied by the I-line. However, with time, a pattern of satiation will develop around the T-line itself. This pattern will be asymmetrical with satiation developing more rapidly in the region furthest from the I-line, i.e. in the initially less satiated region. If the presence of the T-line in the visual field is prolonged, the levels of satiation on either side of the T-line will, it is assumed, reverse and result in displacement towards the area originally occupied by the I-line.

If the above interpretation is correct, it should be possible to induce counter displacement in normal subjects simply by increasing the length of time that the T-figure remains in the visual field. In addition, it can be predicted that variations in the length of inspection of the I-figure will differentially affect counter displacement. Atypical subjects, who exhibit counter displacement after dissipation of the FAE, have a smaller FAE than normals. Since it has been shown that a smaller FAE can be produced by decreasing I-time, it would follow that counter displacement would occur more readily with short than with long I-time.

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Experiment I

The purpose of this experiment was to determine whether, after partial dissipation of the FAE, interpolating a period of prolonged inspection of a T-figure would produce counter displacement. Two lengths of inspection of the I-figure were used to determine whether a shorter I-time facilitates counter displacement.

Subjects

Three men and five women from the staff of the Saskatchewan Training School volunteered to serve in this study. They ranged in age from 20 to 28 with a mean of 24·1 years. None of the subjects was familiar with the task or purpose of the experiment.

Apparatus

The apparatus was the same as reported in a previous investigation (Prysiazniuk and Kelm, 1963). It consisted of two 18 × 8 in. sheets of white cardboard placed one above the other and mounted in an upright wooden frame. The bottom card was stationary but the upper card was secured to an arm which provided movement in a horizontal direction. A knob at the upper left corner which the subject turned provided movement of the upper card through a precision-machined gear assembly. Mounted at the rear of of the apparatus was a vernier caliper from which movement of the upper card could be recorded accurately to within 0·001 in.

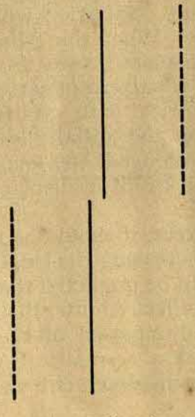
The T-figure consisted of two black lines. Each was 1 in. high and 0·01 in. wide drawn with India ink. One was drawn at the centre-bottom of the upper card and the other at the centre-top of the lower card. With this arrangement the top line could be moved directly above the lower line to form what appeared as one straight line. The fixation- (F-) point was a 0·06 in. aperture located 0·25 in. below and 0·25 in. to the right of the lower line.

The I-figure also consisted of two vertical lines 1 in. high and 0·01 in. wide. They were drawn in India ink on a white 16 × 14 in. card. The lower line as drawn 0·44 in. to the left and 0·25 in. above the centre of the 0·06 in. aperture which served as the F-point. The upper line was drawn 1·25 in. above and 0·035 in. to the left of the F-point. The I-card fitted into a slot directly in front of the T-cards with the two F-points superimposed.

The relationship of the I- to the T-lines is shown in Figure 1. The distance between the lower I- and T-lines was 0·19 in. (visual angle = 54′). The upper I-line was located 0·19 in. to the right of a point at which, on the average, a group of subjects perceived the two lines as forming one straight line (PSE). This point was determined from previous research in which only the bottom I-line was used.

An adjustable chin rest was used throughout the experiment. It was located at one end of a table directly in front of the apparatus so that the distance between the figures and the subject's eyes was always 12 in. The chin rest was adjusted so that the line of vision was perpendicular to the F-point. Room illumination was approximately 32 foot-candles.

FIGURE 1



Schematic representation of I- (dashed lines) and T- (solid lines) figures. The T-figure is shown in the near-aligned position.

Procedure

The general procedure for measuring the development and dissipation of the FAE was as follows. The subject was seated in front of the apparatus with his chin on the rest. The variable T-line was randomly offset to the right of the stationary T-line at a distance ranging from 0·25 to 0·5 in. (If the upper line is offset to subject's left, it would be situated in an initially less satiated region and according to the authors' hypothesis, facilitate counter displacement.) The subject was then instructed to look at the F-point and turn the knob on the left until the bottom and top lines appeared as one straight line. Several trials were given to teach the subject to maintain fixation and to make his alignments within approximately 5 sec. When this criterion was attained the subject was given a 3-min. rest followed by three control alignments to establish the PSE. A rest of 60 sec. was allowed between each of the practice and control alignments. After another 3-min. rest the I-card was placed in front of the T-cards with the F-points superimposed. The subject gazed at the F-point for a defined length of time after which the I-figure was removed. Immediately, the subject aligned the T-lines while still maintaining fixation. Test alignments were also made 30 and 60 sec. after inspection. A period of 3-min. separated the 60-sec. test alignment and the following inspection of the I-figure. Three such inspections of the I-figure and respective test alignments were made followed by a 3-min. rest and three more control alignments.

Conditions

Four conditions were employed in a treatments × treatments × subjects design (Lindquist, 1953). The two variables were length of inspection of the I-figure and presence or absence of prolonged inspection of the T-figure between the 30- and 60-sec. test alignments. In conditions 1 and 3 the I-times were 20 and 40 sec. respectively, and the subject closed his eyes in the interval between alignments. Thus prolonged inspection of the T-figure was not permitted except for the three short periods of time required for the test alignments. In Conditions 2 and 4 I-time was again 20 and 40 sec. respectively, but in these Conditions the subject was required to inspect the T-figure for the entire interval between the 30- and 60-sec. test alignments. That is, after the 30-sec. test alignment the subject continued to stare at the F-point of the T-lines, *as he had aligned them*, for 25 sec. after which the variable T-line was offset and he made his 60-sec. test alignment.

Each subject served under the four conditions which were concluded in four sessions. Each session was separated by an interval of one week. The order of presenting the conditions was randomly determined for each subject prior to the experiment.

Results

There were no significant differences between the three control alignments made before, and the three control alignments made after the experimental trials. Therefore, for each subject the six alignments were averaged to establish the PSE. The three test alignments made at each of the three intervals after inspection were averaged and represented the experimental alignment at each interval. The difference between the mean PSE and mean at each test interval was a measure of figural displacement.

The mean FAE at the 0-, 30- and 60-sec. test alignments under each of the four conditions is shown in Table I. In the control conditions no counter displacement is evident.

TABLE I

MEAN FAE (IN INCHES) AT THREE INTERVALS AFTER INSPECTION UNDER FOUR DIFFERENT CONDITIONS

		Time after inspection (sec.)		
Condition	*I-time (sec.)*	0	30	60
1	20	0·025	0·006	0·002
2	20	0·030	0·005	−0·015*
3	40	0·024	0·010	0·003
4	40	0·024	0·000	−0·010

* $p < 0{\cdot}01$.

However, in conditions 2 and 4, where prolonged inspection of the T-line was given prior to the 60-sec. test alignment, counter displacement occurs. One-tailed t tests for correlated measures between the PSE and the magnitude of counter displacement yielded a value of 3·34 ($p < 0{\cdot}01$) in condition 2 and a value of 1·85 ($p > 0{\cdot}05$) in condition 4. The difference between counter displacement in conditions 2 and 4 was not significant ($t = 1{\cdot}60$; $p > 0{\cdot}05$) indicating that length of I-figure inspection had no effect on the magnitude of counter displacement. This result would seem to contradict the hypothesis formulated earlier that a shorter I-time would facilitate counter displacement. However, an analysis of the FAE for the two I-times indicated that there were virtually no differences at the 0- and 30-sec. test alignments. Thus, the most likely explanation for the failure of shorter inspection of the I-figure to facilitate counter displacement is that the two periods produced only negligible differences in satiation.

EXPERIMENT II

In Experiment 1 it was found that after partial dissipation of the FAE a period of continuous inspection of the T-figure produced counter displacement in normal subjects. However, in previous studies with atypical subjects counter displacement occurred when three short periods of inspection of the T-figure were *distributed* over an interval of 60 sec. While this procedure did not produce counter displacement in normals, it is possible that if the number of such short inspections of the T-figure was increased, counter displacement would occur.

The purpose of this experiment was to determine whether increasing the number of test alignments, and thereby increasing the amount of distributed inspection of the T-figure, would also produce counter displacement in normal subjects. Once again two lengths of I-figure inspection were employed.

Subjects

Six men and fourteen women who were staff members at Saskatchewan Hospital, Weyburn, participated in the study. Their ages ranged from 17 to 41 years with a mean of 23·4 years.

Apparatus

The apparatus was a smaller model of the one used in Experiment 1. It consisted of a wooden frame on the front of which two panels, each 6 × 3 in., were mounted one above the other. The bottom panel was secure but the upper panel was capable of movement in a horizontal direction by means of a knob at the upper right-hand corner. A vernier caliper at the rear of the apparatus recorded the movement of the upper panel accurately to within 0·003 in.

The figures were drawn with India ink on white cards which were then mounted on the panels. The spatial relationships of the I-figure, T-figure, and F-points were the same as in Experiment 1. However, because of a change in the viewing distance, the visual angle subtended by the distance between the I- and T-lines was 36′.

The apparatus was placed on a table below the eye level of the subject with the face of the apparatus tilted approximately 25 degrees from the vertical. The subject sat facing the apparatus with his left elbow on the table and his chin in the palm of his hand. In this position his eyes were 18 in. from the face of the apparatus and his line of vision was perpendicular to the F-point. The illumination of the room was 32 foot-candles.

Procedure

The basic procedure for measuring the development and dissipation of the FAE was the same as in the previous study. Following preliminary practice trials in aligning the T-lines, three control alignments were given. Each alignment was separated by 30 sec. After a 2-min. rest, the subject was given four experimental trials; the first two with 30 sec. and the second two with 60 sec. inspection of the I-figure. Each inspection was followed by test alignments 0, 30, 60, 90, and 120 sec. after inspection. Each trial was separated by a 2-min. rest. Finally, three more control alignments were made—each separated by 30 sec. of rest. The presentation of the two I-times was alternated between subjects in order of their appearance at the laboratory.

Results

The changes in the FAE under the two I-times is shown in Table II. (As in Experiment I, the control alignments taken before and after the experimental trials were combined. In this experiment (and in Experiment III, below) the control settings taken prior to the experimental trials were significantly different from those taken after. The latter showed evidence of counter displacement and therefore reduced the magnitude of this phenomenon shown by the experimental trials.) Analysis of variance showed that there was no significant difference between the two I-times ($F = 1{\cdot}46$; $F_{0{\cdot}05} = 4{\cdot}10$). There was, however, a significant change in the FAE as a function of T-time ($F = 34{\cdot}9$; $F_{0{\cdot}01} = 3{\cdot}44$). The I-time × T-time interaction was not statistically significant.

Two-tailed *t*-tests for correlated measures were carried out between the PSE and the mean experimental alignment on each of the 10 measures of the FAE. Significant counter displacement occurred 120 sec. after inspection in both conditions. Thus, by increasing the number of test alignments and thereby the number of distributed exposures of the T-figure, counter displacement was produced. However, differences in the rate at which counter displacement appeared did not occur under the I-times used. Once again, the most likely explanation is that the range of the two I-times was not great enough for differences to be manifest.

TABLE II

MEAN FAE (IN INCHES) AT VARIOUS INTERVALS AFTER INSPECTION

		Time after inspection (sec.)					
Experiment	*I-time (sec.)*	0	30	60	90	120	150
II	30	0·021†	−0·008	−0·004	−0·005	−0·006*	
	60	0·028†	0·004	0·001	−0·003	−0·012†	
III	15	0·007	0·001	−0·013†	−0·006	−0·004	−0·009†
	75	0·022†	0·003	0·005	0·000	−0·004	−0·009†

* $p < 0{\cdot}05$. † $p < 0{\cdot}01$.

EXPERIMENT III

The purpose of this study was to increase the difference in the length of I-figure inspection and to extend T-time in order to determine whether an extended range of I- and T-time would produce counter displacement at different test intervals. Experiment II was repeated in all respects except that the two I-times were 15 and 75 sec. and an additional alignment was made 150 sec. after inspection.

The subjects were seven men and thirteen women who were staff members at the Saskatchewan Hospital, Weyburn. They ranged in age from 18 to 50 years with a mean of 28·7 years.

The results are presented in Table II. An analysis of variance revealed that the I-time × T-time interaction was significant ($F = 4{\cdot}27$; $F_{0{\cdot}01} = 3{\cdot}11$) indicating that the FAE and its changes with T-time were different under the two I-times.

Two-tailed *t*-tests for correlated measures showed that with an I-time of 15 sec. significant counter displacement occurred 60 sec. after inspection. On the other hand, when I-time was 75 sec. significant counter displacement did not occur until 150 sec. after inspection.

These results thus confirm the previous findings that distributed exposures of the T-figure in the visual field will produce counter displacement in normal subjects. In addition, the rate at which counter displacement appears is increased by providing a shorter period of I-figure inspection.

DISCUSSION

The results of these experiments show that a T-line placed in an area which has just been satiated by an I-line will initially be displaced away from the I-line. If, however, partial dissipation of satiation is allowed, continuous inspection of the T-figure will produce a reversal in the direction of displacement. Alternatively, repeated test alignments which progressively increase the amount of distributed inspection of the T-figure will also produce a reversal in the direction of displacement. In addition a shorter inspection of the I-figure increases the rate at which counter displacement appears.

It is possible that always setting the variable T-line to the subject's right of the stationary T-line may have influenced the results. However, in an earlier study (Springbett and Kelm, unpubl.) in which the variable line was placed to the left and right of the stationary line there was no difference in figural displacement between the two settings. Furthermore, since the strategy in the present study was to determine whether the figural after-effect pattern shown by atypical subjects could be produced in normals, and since in these experiments the variable line was always set to the right, it was decided to retain the same paradigm. After completion of the present investigation three subjects were tested with the variable T-line placed to the left (under conditions of Experiment 1). The results showed the same relationships as those reported.

The explanation of counter displacement outlined earlier rests on the assumption that satiation which develops about the T-line accumulates more rapidly in the originally least satiated region, eventually reversing levels and hence the direction of displacement.

The assumption that the levels of satiation on either side of the T-line will reverse with time is subject to criticism. For, while the growth of satiation is more rapid in the less affected region some satiation must still develop in the more heavily satiated area. And it would seem that the level in this more heavily satiated region will reach its asymptote before the level in the originally least affected area has a chance to become greater.

Thus, the processes involved in counter displacement appear to be more complex than originally assumed. The experiments reported here show that counter displacement results from an interaction between the satiation of the I-figure and that of the T-figure but the exact nature of this interaction remains to be specified.

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EXPECTANCY AND INTERMITTENCY

BY

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When a human subject responds to the second of two closely succeeding stimuli, his reaction time to the second stimulus tends to increase sharply as the interstimulus interval decreases. Controversy has centred on the issue of whether this increase is mainly due to the effects of the first stimulus in producing some kind of block in the central analysing systems or whether it is mainly due to the temporal uncertainty of the second signal, as determined by the distribution of interstimulus intervals used.

By substituting for the first stimulus a spontaneous response on the part of the subject and holding the distribution of interstimulus intervals constant, it is shown that the delays in responding to the succeeding signal are eliminated, even at intervals as short as 50 millisec. This is interpreted as evidence in support of the intermittency hypothesis and as a clear indication that the increase in reaction times normally observed is not a result of the distribution of interstimulus intervals.

INTRODUCTION

A number of investigators, such as Klemmer (1956, 1957), Karlin (1959), Bertelson and Boons (1960), Drazin (1961), Kay and Weiss (1961), have explored the way in which reaction time to a signal varies with the duration of the foreperiod, i.e. the interval between a warning signal and a signal to respond.

Opinions have diverged as to whether the delays in reaction time which are observed when a subject responds to the second of two signals can be explained in similar terms and shown to depend solely on the probability distribution of interstimulus intervals used. Hick (1948), Welford (1952, 1959), Fraisse (1957), Davis (1956*a*, 1957, 1959) have argued that over and above any variation in reaction time due to the subject's uncertainty about the time of arrival of a second signal there exists a "psychological refractory period" during which some phase of the central analysis of the first signal holds up the analysis of the second signal. There are differences between these authors over what they suppose to be the source of the observed delays but they are more or less united in proposing some kind of intermittency in central information handling as the basis of these observed delays.

On the other hand, if it could be shown that the observed values of reaction time depended solely on the probability distribution of interstimulus intervals, the delays in reaction time at short interstimulus intervals could be explained as merely a special case of a more general theory relating reaction time to foreperiod distribution and the hypothesis of a "psychological refractory period" would be unnecessary.

An explanation of this type was proposed by Elithorn and Lawrence (1955) and although this was unsatisfactory in some ways (Hick and Welford, 1956; Davis, 1956*b*) a modified version was proposed by Broadbent (1958) who, on the basis of the evidence available from ungraded response situations, concluded that "the suggestion that refractoriness is a range effect similar to those found in other psychophysical experiments is fairly plausible." Subsequent experiments such as those by Halliday, Kerr and Elithorn (1960), Elithorn (1961), and Borger (1963), have provided much useful information, but there still appears to be considerable disagreement over the main issue of intermittency. One could summarize the differences by saying that those who interpret the results in terms of intermittency attribute the effects to the influence of the first stimulus, whereas those who interpret the results in terms of expectancy attribute the effects to the distribution of interstimulus intervals.

Broadbent (1958) suggested examining the effect of different kinds of distribution of interstimulus intervals, in particular the effect of making short intervals more frequent and long intervals less frequent.

An alternative approach is to reduce or eliminate the effects of the first signal. Kay and Weiss (1961) attempted to do this by reducing the temporal uncertainty of the *first* signal and showed that this produced a corresponding improvement in reaction times to the *second* signal. Such a result is consistent with an intermittency hypothesis, since by making the first signal highly predictable the central analysing systems have no need to process it, and are thus left free for dealing with the second signal.

A more radical demonstration that the effects are due mainly to the influence of the first signal, rather than the distribution of interstimulus intervals, might be obtained by eliminating the first signal altogether and substituting for it a "spontaneous" emitted response by the subject. Thus the subject might be asked to press a key at any moment he chose and this event could be used instead of the first signal, to mark the commencement of the interval. If the distribution of intervals is kept exactly the same as in the more usual situation where two signals are represented, any difference in the pattern of reaction time between the two situations must be attributed to effect of the event which commences the interval, rather than the distribution of intervals used.

An experiment to examine this issue was designed as follows:

METHOD

Apparatus

The same basic apparatus was used as previously described by the author (1957, 1959) with the modification that the circuits governing the appearance of the first signal were completely by-passed and the subject was provided with a trigger key which, when pressed, commenced the timing circuits controlling the appearance of the subsequent signal. This key was operated by the subject's left hand. The subject's right hand operated a key which he was instructed to press as soon as he perceived the subsequent visual signal, a 40 millisec. flash of a neon bulb. Reaction times to this signal were recorded to the nearest millisec. using Dekatron counters.

Procedure

The interval between the subject pressing the trigger key and the onset of the subsequent visual signal was varied randomly from 50 to 500 millisec. steps exactly as in previous experiments (Davis, 1959). Each experimental series consisted of twenty presentations, two at each interval. The subject was told that he could press the trigger key at any time he pleased after the experimenter had indicated that the apparatus was ready. Subjects in fact tended to vary the amount of time they waited after the experimenter had indicated that all was ready from about half a second up to several seconds. The duration of this waiting period was not recorded but there was no question of the subject pressing the trigger key as an immediate reaction to the experimenter's instructions. The pressing of the trigger key had the nature of a "spontaneous" or emitted response on the part of the subject.

The same two right-handed subjects were used a few months after they had taken part in the earlier experiment (Davis, 1959). Each subject performed two practice sessions and the results from 20 experimental sessions were then recorded. If a subject failed to make a response for any reason, the particular interval for which this occurred was repeated later in the session. This happened very rarely and the number of such occasions is shown in the results.

RESULTS

The means and standard deviations of the reaction times at each interval for the two subjects are given in Table I.

These results may be compared with the results for the same two subjects in the earlier experiment (Davis, 1959) where the interval was commenced by a visual signal (a 40 millisec. flash of an adjacent neon bulb). The means and standard deviations of the reaction times in this experiment are reproduced from the earlier paper in Table II.

The difference between the results from the two experiments is immediate and obvious. Whereas in the earlier experiment the reaction times showed the familiar pattern of delays associated with the "psychological refractory period" (a delay of approximately 100 millisec. being observed at an interstimulus interval of 50 millisec.) in the present experiment no such delays are observed. The most extreme variations in mean reaction time over the whole range of intervals lie within a range of plus or minus 10 millisec. If this is an effect of "expectancy" it is vanishingly small. Furthermore an examination of the distributions of individual reaction times showed them to be highly consistent over the whole range of intervals. Further statistical tests seem superfluous.

TABLE I

MEANS AND STANDARD DEVIATIONS OF REACTIONS TIMES (IN MILLISEC.) IN RESPONDING TO A VISUAL SIGNAL WHEN THIS FOLLOWS THE SPONTANEOUS PRESSING OF A KEY AT INTERVALS 50 TO 500 MILLISEC.

N = 40 in every case

Interval (millisec.)	*Subject C*		*Subject D*	
	Mean	*S.D.*	*Mean*	*S.D.*
50	161	16	163	11
100	164	14	167	15
150	156	16	155	12
200	160	12	156	16
250	152	13	151	9
300	150	14	149	10
350	153	10	152	11
400	153	16	149	12
450	155	15	149	12
500	153	15	153	16

Total number of readings requiring repeat for any reason:
Subject C 6 = 1·5 per cent.
Subject D 6 = 1·5 per cent.

TABLE II

MEANS AND STANDARD DEVIATIONS OF REACTION TIMES (IN MILLISEC.) IN RESPONDING TO A VISUAL SIGNAL WHEN THIS FOLLOWS A PREVIOUS VISUAL SIGNAL AT INTERVALS 50 TO 500 MILLISEC.

(taken from Davis (1959))

N = 40 in every case

Interval (millisec.)	*Subject C*		*Subject D*	
	Mean	*S.D.*	*Mean*	*S.D.*
50	254	38	244	48
100	220	33	207	41
150	190	24	181	25
200	176	23	167	13
250	163	20	161	12
300	163	22	161	13
350	164	25	154	22
400	159	18	164	12
450	160	18	150	14
500	152	17	155	10

Total number of readings requiring repeat for any reason:
Subject C 16 = 4 per cent.
Subject D 18 = 4·5 per cent.

DISCUSSION AND CONCLUSIONS

The results seem to establish conclusively that the lengthened reaction times which are normally observed when a subject responds to the second of two closely succeeding stimuli are mainly due to the effects of the first stimulus, and not to the distribution of inter-stimulus intervals.

They are therefore consistent with a form of the intermittency hypotheses which attributes the delays to some phase of the analysis of the first signal interfering with the analysis and preparation of the response to the second signal. The results do not enable one to decide whether the period of intermittency is fixed for any signal requiring analysis or whether it varies according to the amount of uncertainty associated with the signal, although on the basis of other evidence (e.g. Kay and Weiss, 1961) the latter alternative would now seem more likely.

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APPARATUS

A DISPLAY UNIT FOR COMPUTER GENERATED PATTERNS

BY

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A system is described whereby computer generated two dimensional patterns, stored on punched paper tape, can be displayed on an oscilloscope at high speed.

Using computers to generate visual and auditory displays for psychological experiments is well known, White (1962) having reviewed much of this work. One of the most dramatic is that of Julesz (1960) who was able to show many novel features of stereoscopic perception using large fields of randomly distributed data. Such types of display are very difficult to prepare manually.

Another example where preparation difficulties can arise is that involving pattern discrimination. Here, the subject(s) may be required to identify, or match, large numbers of relatively simple patterns (Fitts and Leonard, 1957).

It is relatively easy to programme a computer to prepare such material and also to present the material and analyse the results. The programme parameters may, in some cases, be an indirect representation of certain hypotheses under test. Such a concrete expression necessarily leads to a greater precision of psychological theorizing or modelling.

The main practical barrier to the development of these techniques is the difficulty of providing access to the computer. As White (1962) has said, relatively few computers have a direct visual output. Of those that have a visual output, experimental material may be photographed directly from the display, but this can prove trying as any faults in the photographing procedure is reflected as wasted computer time.

Alternately, the subject can be brought to the console of the computer. However, since much of the machine's time is spent waiting for the subject to respond, this proves to be uneconomic.

If therefore, the computer generated patterns can be stored permanently on tape, and a relatively cheap display unit for reading the tape were available, all the above mentioned difficulties would be overcome.

The purpose of this note is to describe some relatively simple instrumentation using a standard (hence reliable) output medium in the preparation of a visual display. Eight-hole paper tape is conveniently read at high speeds, and is readily transportable and durable.

To generate patterns similar to those of Julesz (1960), a square display is required having a matrix in the order of 100×100. Using alternate tape characters to specify x and y, a display of 128×128 (2^7) may be obtained. The eighth digit of the character (P_8) is used to distinguish between "x" and "y," being present in a "x" character and absent in a "y" character.

The programme is written such that the "y" ordinate is punched first followed by the "x" ordinate.

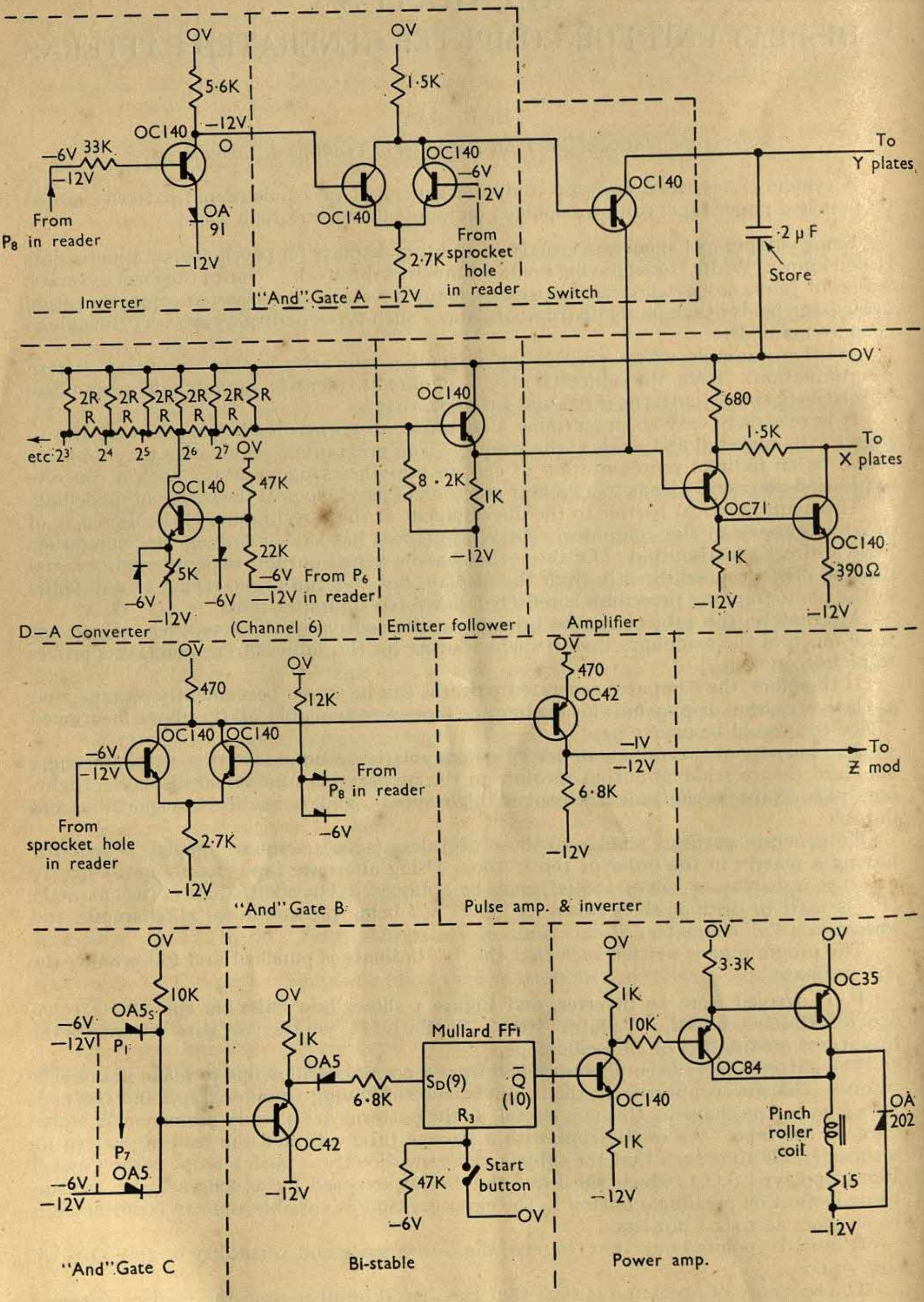
$\bar{P}_8$ is derived from an inverter, and Figure 1 shows how this, via the "&" gate A, controls the switch to the "y plates" store. Similarly, P_8, via the "&" gate B, controls the brightness modulation of an oscilloscope.

The patterns have been photographed from a normal oscilloscope to make permanent records. However, apart from the inconvenience of having to photograph and the need for a viewing mechanism, this is wasteful, as the patterns are already permanently stored once on the tape. An oscilloscope with a storage tube will hold the read-in pattern for a considerable time such that the subject can view directly. Such a scope is the Cawkell Remscope type S.O.1. which will leave the display exposed for as long as 3 hr., erasure being instant on pressing a button. The exposure time is variable and can be adjusted to be as short as 1 sec.; not less.

It must be pointed out, that, to have the convenience and versatility of an S.O.1. will cost £1,000.

The sequence of operation is such that the digital number from the reader, unaccompanied by a P_8, is fed to, via a "digital to analogue" (D-A) converter, and stored on the y

FIGURE 1



Flow diagram of display unit.

plates, using a simple storage capacitor (C). The next number, this time accompanied by a P_8, is fed to the x plates via the D-A converter and the brightness is modulated, imprinting a spot on the 'scope face. The sequence is then repeated, and so building up a pattern.

The tape transport is started by a push button, setting a bi-stable unit. This operates the clutch via a power amplifier.

Stopping the tape between each display is achieved using the existence of all holes from P_1 to P_7 which resets the bi-stable unit. This means the useful matrix is only 127 × 127 instead of a possible 128 × 128.

The sprocket hole on the tape is used to provide sampling pulses to operate the "&" gates A and B.

Requirements for a reader were based mainly on speed. The time to present a display must be very small compared with the time for the subject to respond. Otherwise the completion of an experiment will be slow and the subject is liable to become bored and disinterested. Also the number of experiments in any one session would be severely reduced.

If one considers a rate of 100 dots/sec. (i.e. 200 characters/sec.), the speed of a cheap reader, and a simple pattern of say, 32 dots (Fitts and Leonard, 1957), the time to display the pattern is 320 millisec. (i.e.comparable with simple reaction time). To speed up the display rate by a factor of 5 would, therefore, be advantageous, displaying the above mentioned pattern in 64 millisec.

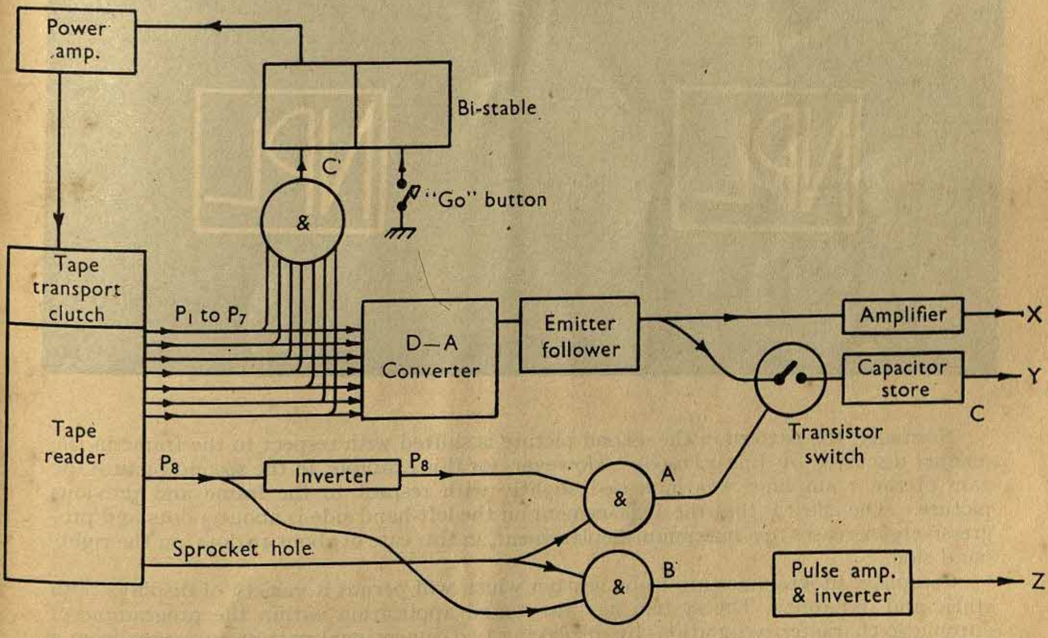
Similarly, to display a stereoscopic pair totalling 10,000 dots, would take 1 min. 40 sec. at 100 dots/sec. compared with 20 sec. at 500 dots/sec.

Tape readers working at this speed are very expensive, due mainly to sophisticated stopping mechanisms which were not required for this purpose.

The reader chosen was an Elliott Tape Reader type D4/82 whose normal speed is 200 characters/sec. This machine at the time of writing, cost only half of that required for the faster machine.

A direct replacement double speed motor was fitted, and with two very minor modifications, a large capstan fitted. The result, was a reader whose speed was 1000 characters/sec. for little more than £300.

FIGURE 2



Circuit diagram of display unit.

A higher display rate can be achieved in two ways. One, by a little more modification to the reader, an even larger capstan can be fitted. Secondly, by having two D-A converters, one for x and one for y, the y converter being made up with bi-stables such that it will store a number indefinitely. This means that y need only be stated once for any number of x values. When producing random noise patterns such as Julesz (1960) the display rate would be increased by as much as 40 per cent. Bearing in mind that to produce a pair of stereoscopic patterns at 1000 characters/sec. would take 20 sec., the extra speed might be desirable.

Considering using the display unit dynamically, in most experiments the addition of bi-stables would have no relevance, and timing would be achieved by spacing the characters out along the tape to introduce the appropriate delay between dots.

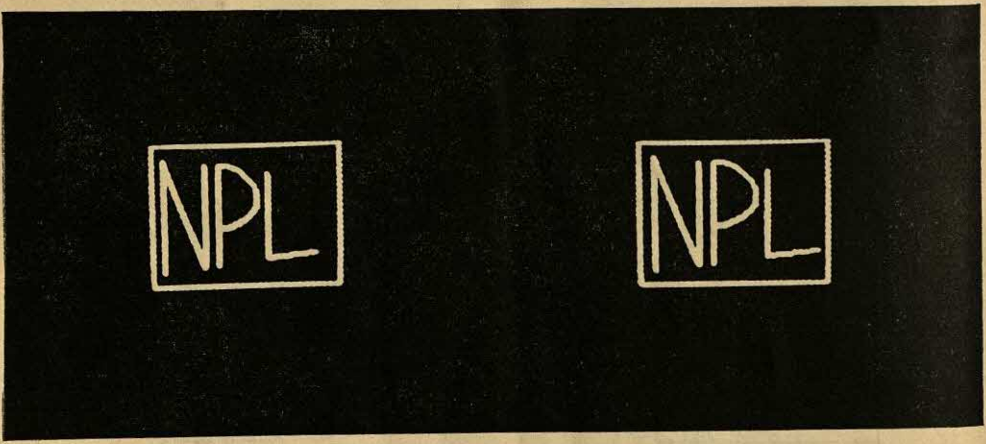
The problem of bulk tape had also to be considered. Information to produce 10,000 dots (i.e. 20,000 characters) require nearly 200 ft. of tape. To ensure that the inertia of such a bulk does not interfere with the running of the tape through the reader, an efficient pay out device is required. Such a mechanism has been developed.

The complete circuit diagram of the equipment is shown, but some explanation of the y store is required. The charge on the capacitor C tends to discharge through the input impedance of the 'scope (1 meg ohm) and to ensure a long discharge time with respect to 1 millisec., the capacitor was chosen accordingly, i.e. $0{\cdot}2\ \mu F \times 1$ m. = 200 millisec. However, the rise time, influenced by the output impedance of the D-A converter, must be very short. The output impedance of the D-A converter proved to be too high, so the addition of an "emmitter follower" (i.e. an impedance transformer) was necessary to give the desired results.

The resistors marked "R" in the D-A converter were within ± 1 per cent. of each other and the linearity and repeatability of the pattern was better than ± 0·5 per cent. and ± 0·1 per cent. respectively.

By way of psychological evaluation, a novel steroscopic display has been prepared which is illustrated below.

FIGURE 3



Normally, the pattern in the second picture is shifted with respect to the frame in the manner discussed by Julesz (1960). However, for this example, in the second picture the gain of the x amplifier was increased slightly with respect to the frame and previous picture. The effect is that the displacement on the left-hand side is about 3 dots and progressively increases to a maximum displacement, in this case of about 10 dots, on the right-hand side.

Computer programmes are to be written which will permit a variety of displays both static and dynamic. The system has also found application within the programme of automatic character recognition, by displaying two dimensional pattern processes from a digital computer.

The work described above has been carried out as part of the research programme of the National Physical Laboratory, and this report is published by permission of the Director of the Laboratory. The author desires to acknowledge the valuable discussions with Mr. D. O. Clayden, Dr. M. B. Clowes and Mr. J. R. Parks, also to thank Mr. C. F. Osborne who developed the tape dispenser.

Note: Since writing this paper, the author wishes to point out that silicon transistors have been greatly developed and their use would improve the performance and contribute to greater reliability. Furthermore, the NPN types are considerably cheaper.

The author will be pleased to advise anyone contemplating building such a piece of equipment.

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PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY SOCIETY, 1964

7th–9th April, 1964. Extended Meeting at Cambridge.

1st Session: "The third and fourth dimensions," by R. L. Gregory. "Binocular masking induced by movement," by G. C. Grindley and V. Townsend. "Investigation of a cue used in the auditory detection of obstacles," by J. P. Wilson.*

2nd Session: "A formulation of the detrimental effects of over-high or over-low motivation upon performance," by D. E. Broadbent. "Quantum efficiency of vision with different psychophysical methods," by A. H. Gregory.*

3rd Session: "Multiple or fractional magnitude estimates?" by E. C. Poulton. "Learning to learn," by L. J. Postman. "Interaction between brief asynchronous visual stimuli," by M. Kinsbourne and E. Warrington.

4th Session: "Experimental investigations of wave-spike discharges," by B. Tizard.* "Attenuation and recovery of response to stimulation of cells in the brain stem of the rabbit," by G. Horn.

5th Session: "The projection of the retina on to the cerebral cortex in the squirrel monkey," by A. Cowey. "Visual analysis in temporal primates," by S. D. Iverson.* "Memory impairments of the frontal and temporal Baboon," by A. W. H. Buffery.*

9th–11th July, 1964. Extended meeting at Oxford.

1st Session: "Weber's Law and signal detection," by M. Treisman and J. Clarke. "Experiments on understanding sentences," by J. Marshall.* "Ignoring irrelevant information," by P. M. A. Rabbitt.

2nd Session: "Temporal factors in pattern vision," by A. Fiorentini and D. M. MacKay. "The conditions necessary for visuo-motor adaptation to optically displaced vision," by I. P. Howard, M. B. Craske and W. B. Templeton.

3rd Session: "The visibility of brightness fluctuations," by C. Rashbass.* "The effect of uncertainty of response on perceptual recognition thresholds," by P. Fraisse.† "Is there any relation between stimulus discriminability and response discriminability," by J. F. Richard.†

4th Session: "The role of the osmotic pressure and volume of the extracellular fluid in thirst," by K. Oatley.* "The genesis of concepts," by E. Vurpillot.† "Memory in relation to the frequency and familiarity of linguistic elements," by G. Oleron.†

5th Session: "Spreading depression and behaviour," by I. S. Russell, R. B. Ross and K. T. Strongman. "Integration of simple visual concepts in the brain-stem of split-brain monkeys," by C. Trevarthen.* "Self stimulation in pigeons," by E. M. McPhail.

4th–6th January, 1965. 17th Annual General Meeting at the National Hospital for Nervous Diseases, London.

1st Session: "Visual guidance of the forelimbs in Rhesus monkeys," by J. Lund.* "Investigation of the tactile cross-placing reaction in Rhesus monkeys," by J. P. Lumley.* "The effect of large cortical lesions on visual learning in monkeys," by C. R. Butler.* "Frontal lobe lesions and monkeys' stimulus sampling," by L. Weiskrantz.

2nd Session: "Structure-activity relationship studies of Mescaline analogues on the conditioned avoidance response in rats," by J. R. Smythies and Dr. E. A. Sykes. "Perception of stimulus relationships in animals," by D. A. Riley.

3rd Session: "Retention of letter-word paired associates," by N. S. Anderson. "A disorder of simultaneous form perception," by M. Kinsbourne and E. Warrington. "Ideational apraxia," by E. Warrington and O. L. Zangwill.

4th Session: "Methodological considerations of discrimination learning," by I. S. Russell and K. T. Strongman. "On the ability of subjects to follow small local variations of bias in a self-paced choice reaction task," by J. A. Leonard and R. C. Newman.

5th Session: "The effect of attention on the responses to thalamic stimulation in man," by J. A. V. Bates.*

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W. B. Webb, Ph.D., Psychological Laboratory, Downing Street, Cambridge.
S. B. Williams, Ph.D., Psychological Laboratory, Downing Street, Cambridge.

* By invitation. † Special guest from France.

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Comunicarea in Procesul Muncii. By Tatiana Slama-Cazacu. Bucarest: Editura Stiintifica, 1964. Pp. 300. Lei. 12.

BOOK REVIEWS

Traité de Psychologie Expérimentale. Edited by Paul Fraisse and Jean Piaget. Nine Volumes. Paris: Presses Universitaires de France. 1963.

Volume I. Histoire et Méthode. By Jean Piaget, Paul Fraisse and Maurice Reuchlin. Pp. 121. 12F.

This is the first volume of the new *Traité de Psychologie Expérimentale,* which will comprise a further eight—not far short of a million words in all. The undertaking is bold and sizeable and we must be grateful for the energy and industry of our French colleagues who have produced so rapidly something near a finished product to lay alongside the projects and discussions which are wont to simmer on in other countries. Now at least we have a basis for trying to answer such questions as: "Is it possible to produce a co-ordinated account, on this scale, of contemporary experimental psychology neither diffusely prolix nor clogged with particularities of ephemeral interest?"; "Towards what class or classes of readers can such a work be directed?"; "Is the outcome of so much labour likely to be a monumental show-cabinet rather than a living aid to teaching and research?"

These questions cannot be answered here, but the mere fact that this first volume is devoted to History and Method raises others. Treatises on, say, zoology and physiology do not commonly start with these topics. History is sometimes briefly outlined where it can illuminate the present position of theory or research, method is usually discussed only in special technical connections. As to abstract matters such as that of the *type* of explanation appropriate to the subject, these are generally left for discussion elsewhere. Why then do we find the editors of a presumably corresponding treatise on psychology clearly judging these topics essential as a starting point? The unsympathetic critic has a forthright explanation. This harping on history, method and philosophy, he says, is so much defensive bolstering against charges of inadequate or non-existent scientific achievement. But the matter is not so simple or straightforward as this. Historical and methodological preoccupations are not there as a substitute for scientific achievement, of which much can be discerned. They serve, rather, to sustain the nominal unity of an academic discipline threatened by stresses resulting from unequal development of its various parts and from the changing relationships of some of them with other neighbouring fields. Only by appeal to some common historical theme, some identity of methodological approach or some single question of philosophical significance can such a diverse collection of interests and activities be brought under the single title "Psychology." This is no happy state of affairs. Whether it can continue, extended treatises such as the present may have little part in deciding.

Misgivings such as these, however, should not bias appreciation of the actual contents of this volume. The first, long, chapter by Professor Fraisse on the evolution of experimental psychology can of course present no more than a brief and selective summary, but is fair and informative, if rather along lines laid down by previous historians. The struggle psychology had to establish its claim to scientific status and the dominant part played by a few individuals have tended to create a conventionalized myth from which it is now difficult to escape. A fresh look at the development of ideas in the nineteenth century could be rewarding.

In Chapter II Fraisse lays out with impeccable care and clarity the principles of experimental procedure with special emphasis on features peculiar to psychology. Once again, one cannot help wondering a little for whom such an exposition is intended. The advanced student will know it all, though he might not be able to write it down so well. The beginner might be misled into supposing that all he need do to make significant contributions to the subject is to follow the rules, with the danger that adherence to them may restrict his insight into the problems. To Fraisse himself, it evidently seems that insistence on canons of method is a necessary bulwark against hasty judgment in the solution of practical problems. "D'audacieuses prudences permettent que la pratique et la science se rejoignent peu à peu," he concludes. Has not the time come for a prudent resort to a little audacity?

Professor Piaget's chapter on "Explanation in Psychology and Psychophysiological Parallelism" affords him the opportunity of adding yet another item to the corpus of work for study by those anxious to follow and understand his thinking. Close examination of various types of explanation in psychology leads him to the hope of an isomorphism between physiological reductionist schemas of explanation and the schemas generated by logico-mathematical procedure. Many will share his hope, though some may doubt whether the concept of isomorphism, at any rate as derived from the paradigm of similarity between the two visible pictures, is as clear or as ultimately useful as it may seem at first sight. But certainly Piaget's contribution forcibly emphasizes once again the difficulty of reaching any satisfactory explanation of explanation in psychology.

The final chapter by M. Reuchlin on "Measurement in Psychology" is cast quite largely in the framework suggested by Stevens. It does no more than touch on the more sophisticated techniques which have so rapidly developed in the last fifteen years on such bases as information- and signal detection-theory. It is difficult to follow contemporary experimental literature without some notion of these, though doubtless the decision whether or not to include them must have been a difficult one. But here we return to our original question. What is the function of a treatise on this particular scale? It would be quite unfair to answer this without full consideration of the whole work. Yet one is left with the impression that it is not massive or detailed enough to serve as a reference book. On the other hand, if the intention is to provide a manual of instruction, doubts may remain as to whether this is the ideal form for it to take. In either event, the courage and skill of editors and contributors deserve the warmest tribute.

R. C. OLDFIELD.

Volume II. Sensation et Motricité. By Henri Piéron, René Chocholle and Jacques Le Plat. Pp. 159. 12F.

From the title of this volume one might have expected a text on sensory and motor mechanisms. Instead, however, we are provided with three very useful independent essays, by Piéron on "La Psychophysique," by Chocholle on "Les Temps de Réaction" and by Le Plat on "Les Liasons Sensori-motrices."

After a brief account of methods for measuring absolute and differential thresholds, and a glance at the form of the psychometric function, Piéron settles down to his main theme. This is the problem of psychophysical scaling, and in particular the relative merits of Fechner's logarithmic law as compared with the power law, originally proposed by Plateau and of late propounded with such ardent conviction by Stevens. Piéron makes his own position quite clear. For him "La psychophysique objective" is based on the determination of thresholds and leads to scales derived by the cumulation of j.n.d.s. Other procedures, such as magnitude estimation, partition and category scaling, belong to "la psychophysique subjective." Stevens, of course, takes the opposite point of view and regards scales obtained by the "direct" methods as more objective than scales derived from j.n.d.s.

Instead of attaching evaluative labels of this kind, it might be better to distinguish between what is a matter of empirical content and what is a matter of definition. The nature of the Weber relationship between j.n.d. and stimulus value is an empirical matter and on this issue Piéron gives an admirable summary of the evidence. The derivation of Fechner's law requires the assumption that the j.n.d. is used as a unit to measure sensation and this seems to be a matter of definition. In the "direct" methods the relationship between the stimulus value and the subject's response is empirically determined. Here again Piéron gives a valuable review of the evidence, pointing out that the results obtained are highly variable, both within and between subjects and that they depend considerably on the method and order of presenting stimuli. However, in order to derive sensation scales from this data, a further step is necessary and the assumption usually made is that the subject's response is directly equated with the magnitude of the sensation he experiences. This, again, seems to be a matter of definition. As Treisman has recently pointed out, there are really two relationships involved, one between the physical stimulus value and the sensation value, the other between sensation value and response. By making different assumptions about the relationship between sensation value and response one can derive a logarithmic law from the "direct" methods and a power law from j.n.d. data.

This chapter should be widely read, however, for the discussion of the empirical issues.

Chocholle, in his chapter, brings together the results from a multitude of experiments in which reaction times have been measured. In doing this he seems to have deliberately refrained from presenting much of a theoretical framework. The result is that, for a research worker who knows what he is looking for, the chapter is a mine of useful information, but someone who is using the *Traité* as a guide to contemporary thinking in experimental psychology may very well fail to see the wood for the trees. Furthermore, of the

396 references given in the text, all but 68 have been cut from the bibliography. Those of us who have seen the original bibliography will remain eternally grateful to Chocholle for his painstaking work in collecting and presenting this material. May we plead with the editors to restore it to its original form in future editions, so that a wider public may derive the benefit of Chocholle's work?

Le Plat's chapter deals with the field commonly known as "Human Engineering." He states that his intention is to deal with the application of experimental methods to practical problems, and this he carries out well in his discussion of topics such as stimulus-response compatibility, pursuit and compensatory tracking. One might have hoped for a fuller discussion of theoretical issues, for example the notion of channel capacity, which in various forms has influenced the course of much research during the last decade. However, within the limits he sets himself, this is a very useful survey.

In summary, this volume should attract many readers, and interest in it will not be confined to French speaking communities. Furthermore, in common with the other volumes of the *Traité*, and in contrast with many publications in the English language, it is remarkably good value for money.

R. DAVIS.

Volume V. Motivation, Émotion et Personnalité. By Joseph Nuttin, Paul Fraisse and Richard Meili. Pp. 226. 15F.

It is to be expected that this series will be widely used as introductory texts by French-speaking undergraduates, partly because of the presentation in separate paper-backed volumes, and partly because of the high standing of the general editors and authors. This volume is clear and simple in its layout, and the few illustrations all contribute something. However, the main point of a review is presumably to compare these texts, which are likely to become standard, with comparable English-language texts. How do syllabuses compare? The nearest equivalent is the *Foundations of Modern Psychology* series (general editor R. S. Lazarus, New Jersey: Prentice Hall Inc.) which covers similar ground in 13 volumes, and which has appeared roughly a year later. In the volume under review, the sections on motivation (Nuttin) and emotion (Fraisse) are comparable with the Prentice Hall volume *Motivation and Emotion* by E. J. Murray. The lengths are similar and so is the level of writing. The striking thing is that the French and American books are very largely in agreement over content. There are only slight differences. The French book deals very well with "knowledge of results," the American omits it. The American has a whole chapter on sex, the French book very little. But the similarities are most apparent, even to the extent of similar subtitles for paragraphs. It is surprising to find such intercontinental agreement on topics which are perennially controversial in undergraduate seminars. The differences between the books become apparent only when one compares the experiments which are quoted. The American references are on the whole to more recent, and often better, experiments. The same emphasis on recency can be seen in the sections on theories. Professor Fraisse is excellent on James's theory of emotion (and is clear regarding its essential untestability) whereas Professor Murray says little about it, and spends more time on the theories of Cannon and Magda Arnold.

The comparison makes it clear that nowadays it is difficult to deal with motivation and emotion in separate chapters. Many topics have had to be allotted arbitarily. The Yerkes-Dodson law is dealt with under emotion; conflict and frustration under motivation. The American author has the advantage of being able to deal with both topics in a systematic and interrelated way.

In the French volume, Professor Meili has written the section on personality. The relevant book in the American series is not really comparable, since it sets out to deal also with adjustment and mental health. Meili, on the other hand, has very little to say about abnormal psychology, and the book probably suffers from this lack. It is in fact weakest on those aspects of the psychology of personality which have been influenced by the comparative method. For instance there is nothing of importance deriving from cross-cultural studies, and the section on personality development is underweight (although there are useful references to Meili's own excellent work). The contributions of genetics and learning theory are hardly referred to. However the author must have had problems of selection in providing an introduction to personality in only thirty thousand words, and he has confined himself to talking mainly about the Structure of Personality, which is the full title of his contribution. His critical exposition of the various factor analytic studies, especially those of Cattell and Eysenck, is first rate, and there are useful introductions to typologies, traits and the various theories of the ego and its development.

All three authors of this volume have set out to provide surveys rather than to sytsematize. They have succeeded in doing so without giving the appearance of merely cataloguing their subjects. The book has over 600 references about three-quarters of which come from American sources. There is no index, which is inexcusable in an introductory text.

B. M. FOSS.

Volume VI. La Perception. By Jean Piaget, Paul Fraisse, Élaine Vurpillot and Robert Francès.

The name *Traité* inevitably conjures up in the English mind the notion of an extensive and exhaustive survey of the subject as a whole. The section in *Perception* in this *Traité* is certainly not of this nature. It consists of four chapters by different authors in which they have apparently written about those aspects of perception which interest them. The chapters are not co-ordinated together; consequently there is considerable overlap, and some important topics have been omitted altogether.

The chapters by Piaget, Fraisse and Francès present in shortened form the contents of books previously published by these authors: *Les Mécanismes Perceptifs, Psychologie du Temps* and *Le Développement Perceptif*. The reader who does not wish to peruse these books in full may find these summaries useful, but he will discover little that goes beyond them. As may be imagined, the chapter by Piaget is devoted to the experimental work of Piaget and his collaborators. It discusses the phenomena of centration, field effects and perceptual activity, in relation to age changes in the perception of visual illusions, constancy and apparent movement; and stresses that there is little maturational development in perception itself, but that perception becomes objectively accurate only when it is organized within cognitive schemes, and controlled by them. Here Piaget is dealing only with restricted experimental situations, and is no longer concerned with the unrestricted perceptions of everyday life which he earlier studied in infants to such good purpose.

The other three chapters have a somewhat more general scope. The chapter by Francès might indeed be regarded as a useful elementary introduction to the experimental study of perception. After making the important distinction between the relatively simple process of discrimination between shapes and the more complex and highly developed identification of shapes and objects (a distinction which Piaget seems to disregard), Francès gives a short conventional outline of the perception of shape: the Gestalt effects, and the influences of learning, field factors, frequency and probability. Although he alludes to information theory, he does not mention, for instance, Garner's recent important discussion of uncertainty, which is very relevant to the distinction between discrimination and identification. The only novel features are the descriptions of his own experiments on hearing. The effects of meaning, etc., on the identification of objects are outlined, and there is a final brief section on motivation and interest.

Vurpillot gives a fairly comprehensive account of the phenomena of space perception, which might again provide a clear introductory treatment of this subject. The discussion of the interaction between visual and other sensory data is particularly valuable. But there is throughout a tendency to omit conflicting data which are difficult to reconcile with each other, for instance, on the functions of convergence and on the Ames equivalent room.

Fraisse presents a topic, the perception and estimation of time, which has little immediate connection with the other contents of the section. But readers may find it useful to have a summarized account of the experiments of Fraisse and his colleagues on the estimation of time, although as he points out, only very short durations can be said to be directly perceived, estimates of longer durations depending on cognitive inferences about sequences of events. Conditioning and rhythm are briefly considered, but not the temporal aspects of form perception, as in figural after-effects, after-effects of perceived movement, etc.

Thus readers who are unfamiliar with the work of French-speaking psychologists may find interesting material in this section; references to the original experiments are particularly valuable. But the section does not provide a comprehensive treatment of experimental work on perception; nor any very novel and stimulating suggestions as to its future development. It is largely orientated towards the work of the Piaget school. As perhaps the title indicates, there is little general theoretical discussion.

M. D. VERNON.

Volume VII. L'Intelligence. By Pierre Oléron, Jean Piaget, Bärbel Inhelder and Pierre Gréco. Pp. 208. 14F.

This volume is likely to prove of considerable interest to English speaking readers in that much of it is devoted to an area of research the origin of which is Swiss rather than American, so that existing handbooks in English offer little to compare with it.

The first chapter, by Oléron, is a general review of experimental research on what is sometimes loosely termed thinking. He calls his chapter "Les activités intellectuelles," thus proclaiming a frankly behaviourist approach, but one which turns out to be far from intransigent. Oléron holds that experiment provides a surer ground than theory for an appraisal of what has been achieved in this field. If the theoretical frameworks that have guided the various experimenters turn out to be somewhat disparate their findings are none the less worthy of consideration in their own right. His review is commendably full and up to date, although there are a number of omissions. Among these I noted Bartlett's work on thinking and Bruner, Goodnow and Austin's on cognitive strategies (both of which are, however, included in his bibliography), as well as Festinger's work on cognitive dissonance (which is not). It would be difficult, and perhaps impossible, for such a review to be exhaustive, and Oléron has preferred to devote the available space to a fuller discussion of selected studies. Thus, both in his discussion of work on induction (e.g. concept attainment) and in his treatment of problem solving, Oléron draws a clear distinction between situational and subject variables, one which is not unhelpful in attempting to sort these rather tangled fields.

The two central chapters are by Piaget and Inhelder. The first of these is something of a departure from tradition, being devoted entirely to the topic of imagery. Although there are useful references to recent neurological work on the subject, the bulk of the chapter consists of a fairly detailed account of the writers' own work on imaginal development. Piaget's conception of the image is somewhat broad and operational. The Geneva experiments on the development of imagery are novel and their results are often unexpected. The evidence that imagery cannot account for the emergence of intelligent insights seems incontrovertible: the authors offer a number of convincing demonstrations that images (e.g. of transformations) which are quite obvious to the sophisticated adult are absent in young children. There is less evidence for the authors' contention that imagery is a form of interiorized imitation found only in man and the higher primates, and that it does not occur in human beings before the age of 18 months. The reader may regret that there is little reference to problems of interpersonal differences in imagery, the role of imagery in creativity and invention, and so forth.

The third chapter is a masterly sketch of the writers' main life-work, concerning the development of operational behaviour, i.e. of those systematic implications which may be inferred from the behaviour of subjects when confronted with problem situations. This is not the first compact overview of Piaget's work, but, not surprisingly, it is the best. We have clear accounts of an adequate selection of the most representative experiments illustrating each of the principal developmental levels, and the discussion brings out admirably the reasons which prompt the authors to regard such behaviours as characteristic of distinct stages in reasoning. Those who are familiar with the work will note with appreciation that Inhelder and Piaget are now in a position to provide ample quantitative information in respect of nearly all the experiments quoted, most of this appearing in print for the first time. This new evidence has clearly inclined Piaget to modify some of his earlier formulations, which often made little allowance for individual variation either in the subjects, or, more particularly, in the experimental situation. Nevertheless, his overall conclusion remains as it was: while maturation and environmental opportunity both play a part in the development of systematization, neither is sufficient to account for the form that it takes, nor can this be satisfactorily explained by appealing to the formative influence of language. The intrinsic ground for this development, he argues, is the law of equilibration, i.e. the tendency for poorly articulated and partial strategies to give way to more articulated strategies, adequate to a wider range of situations. "Such transformations invariably arise in response to problems, lacunae and conflicts. In other words, what triggers the change is an initial disequilibrium; an operational solution restores the equilibrium" (p. 152).

The last chapter is a brilliant essay by Gréco on the relation between problem solving and learning. His discussion of relevant studies leads him to distinguish (1) learning where the action of reinforcement is to confirm or infirm "hypotheses" which ultimately derive from systematic structures governing the apprehension of experience; and (2) learning which consists in the acquisition of these structures themselves. Gréco takes these to be distinct kinds of learning, while allowing that concrete instances of learning may represent a continuum on which successive points are specificed by the relative potency of these two modes.

Taken as a whole, the volume is full of interest, and often provocative. Piaget repeatedly stresses the affinities between his own viewpoint and that of cybernetically oriented psychologies. Such affinities would have been clearer if the present volume had included a review of psycholinguistic work which is doubtless included in Volume VIII. In the opinion of the present reviewer, Piaget's easy dismissal of the role of language in reasoning is largely due to a narrow interpretation of language and linguistic processes.

Finally, one is bound to regret the fact that a volume devoted specifically to problems of intelligence should leave entirely out of account all that has been gathered in respect of individual differences in general intelligence and specific abilities by psychometric procedures. Clearly such techniques are judged to be non-experimental. Nevertheless, the data are probably at least as concordant as those obtained by experimental methods and they are certainly relevant to any discussion of the processes which give rise to them.

E. A. LUNZER.

Natural Language and the Computer. Edited by Paul L. Garvin. London and New York: McGraw Hill. 1963. Pp. xii + 398. 99s. $12.75.

This rich (and costly) table is not spread for psychologists. It is intended, and will usefully serve, to introduce engineers and physicists to current problems and methods of mechanically handling information expressed in natural language. Crumbs and pickings of psychological interest can be had throughout; but since interest in semantic aspects of language is sparse, and the word "understanding" does not appear once in a (very good) 15-page subject index, it would be foolish to look here for much on such problems as the *comprehension* of natural language by artefacts.

As one of the symposiasts succinctly puts it, for him computers "are not magical and are not 'geniuses.' They are better classified as high speed, reliable 'idiots' " (p. 105). Must they always be so? The question is scarcely raised here. A chapter by W. Karush "On the use of mathematics in behavioral research" sounds promising; but it turns out to be mostly about mathematics and hardly at all about behaviour. When the connection is made, it is apparently in the interests of ". . . exploiting the behaviour of biological organisms in the study of self-adaptive and intelligent behaviour of computers." But no one in the symposium takes us very far along this road in the field of natural language.

Perhaps nearest to doing so, at least implicitly, are M. E. Maron's "Logician's view of language-data processing" and the other papers on problems of mechanical informational retrieval. R. M. Hayes' provocative comparison of the human mind with a "self-organizing, multiple-level file," supported by quotations from Hadamard, may be found particularly stimulating. Notions such as (statistically) "weighted indexing," for example (p. 290) might well find an echo in the theory of human remembering.

On the whole, however, those of us with psychological interests had better wait for stage two: when men get tired of "idiot" machines, and try to get their creatures to understand what language is about. Then—and it may not be long—there should be pickings for all.

D. M. MACKAY.

Conditioning and Enuresis. By S. H. Lovibond. Oxford, New York, London, Paris: Pergamon, 1964. Pp. v + 219. 50s.

Some clinical psychologists have made such large claims for the results achieved by conditioning methods in the treatment of nocturnal enuresis that readers of this monograph will be surprised that the author, a clinical psychologist, speaks of his results, as "frustratingly indecisive." The last chapter on "Adjuvant Therapy," moreover, returns to drug methods, bladder exercises and "perineal exercises and faradism," and ends up with the comment that "it would be as well to refrain from undue optimism as the history of the treatment of enuresis is replete with examples of therapies which do not live up to their early promise."

However, this is not a fair summary of the author's views. He makes the customary attacks upon "clinicians," especially those who (like the reviewer) have questioned the evidence relating to the therapeutic effectiveness of conditioning methods, and shows the usual indulgence towards the weakness of the methods and theory of the Maudsley psychologists. There are two points of interest about his own results. Firstly, he achieves an "arrest" in all cases, although sometimes not until after 50 "stimuli" have been given. Secondly, a relatively large number—about one-third—relapse after a month or more. One difficulty in making trials of treatments of enuresis is that a substantial proportion of cases clear up after the first one or two interviews and before the trial has begun, whatever theory the clinician may subscribe to. Reading between the lines, one suspects that Dr. Lovibond met difficulty in assembling adequate samples for his trials and that they were composed of relatively intractable cases, although he accepted a case if the bed wetting frequency was at least three times a week. One would have liked more details of the manner in which he collected his cases. Less than one-quarter cleared up with less than 10 stimuli. It remains questionable, therefore, whether the remissions he achieved came about through the mechanisms he attributes them to.

Librarians and private readers may feel some reluctance in paying 50 shillings for a monograph of this size. This reviewer sees no reason to persuade them to pay up. It adds little that is not contained in other recent reviews. Most of the data have already been reported in the journals.

D. Russell Davis.

Frustration and Conflict. By Aubrey J. Yates. London: Methuen. 1962. Pp. x + 236. 28s.

This work may be characterized as an extensively annotated bibliography. Four of the chapters are reviews of special topics which have each given rise to a considerable number of experiments: frustration and fixation; frustration and aggression; frustration and regression; and conflict. The last chapter is a brief and loosely organized essay which as well as attempting to relate the two concepts of the title also includes comments on terminology and mentions some of the more recent uses of the concept of frustration in theories of learning and motivation.

The four review chapters comprise the most useful contents of the book. The literature has been very thoroughly explored and not only are most of the relevant experimental results brought together but a brief yet lucid commentary is provided. Each of the topics springs from a classic source: Maier's *Frustration;* Dollard and others' *Frustration and Aggression;* Barker, Dembo and Lewin's *Frustration and Regression;* and Miller's *Experimental Studies of Conflict.* It is therefore not surprising to find that for the most part it is the ideas of these men which provide the basis for the systematic survey and analysis of results given in these chapters. Indeed the author explicitly lays no claim to be making any original theoretical contribution.

However, this lack of an expressed personal viewpoint makes the book hard to read because one is exposed to nearly all the relevant literature in a very short space of time. Also the chapters are self-contained and there is no thread of argument which links them. Furthermore one could wish for more contact to be made with ideas from outside the central themes: for example, in the study of conflict, a reference to those less extreme conflicts which are involved in all choice behaviour.

But this is to criticize the author for not producing an altogether different book. As an introduction to the research literature on frustration and conflict this one will prove quite useful.

R. J. Audley.

Cerebral Function in Infancy and Childhood. By Albrecht Peiper, M.D. Translated from the third revised German edition by Benedict and Hilde Nagler. The International Behavioral Sciences Series. Edited by Joseph Wortis, M.D. New York: Consultants Bureau. 1964. Pp. xii + 683. $25.0.

This very large and very German work is meant for paediatric neurologists and others interested in the neurology of early infancy. It may also be of some use to psychologists, in spite of the author's assertion that "child psychology . . . has been completely unproductive in this area of knowledge." The approach is very much in the tradition of Magnus, de Kleijn and Rademaker, and with a strong evolutionary bias. There is a chapter on conditioned reflexes. Although the tone is disagreeably didactic, this book really does contain much important information and many odd and out of the way observations, e.g. that the invention of the pram in England in the middle of the last century gave rise to widespread indignation among pedestrians. This is the sort of book that is often described as "monumental," though quite what this means is perhaps better not pursued.

O. L. Zangwill.

Journal Français d'Oto-rhino-laryngologie, XI, 6 *and* 7, 1962. Colloque: "Intelligibilité et Prothèse auditive."

These two issues of the journal are devoted to papers given in French at a symposium on Intelligibility and Hearing Aids in Deafness, held by the Société Française de Phoniatrie in Paris in 1961. The papers in the first volume present, for an audience consisting mainly of clinicians, speech therapists and hearing-aid specialists, the concept of intelligibility and recent research in hearing, communication and speech analysis. Much of the work parallels that done in the States. Most of it will be familiar to psychologists, or can be found more clearly and concisely explained in books by Cherry, Miller, Fletcher and others. The most novel and valuable paper is probably that describing the oscillographic analysis of speech by Madame Borel-Maisonny. The later papers are devoted to applied problems and remedies for deafness.

Anne Treisman.

A Pharmacological Approach to the Brain from its Inner and Outer Surface. By W. Feldberg. London: Arnold. 1963. Pp. 128. 18s.

The existence of the blood-brain barrier has been one of the salient difficulties in the pharmacological study of the effects of drugs on the brain. The brain and the c.s.f. appear to be protected in important biochemical respects and maintained in more complete homeostasis than other tissues of the body. Because of this, the direct effects on the brain of many pharmacologic agents, whose systematic effects are known, have not been specified. Professor Feldberg's book describes a cannula technique that he has developed to enable him to by-pass the blood-brain barrier. Using permanently indwelling cannulae giving access to the brain ventricles, injections can be made directly into the c.s.f. of the conscious animal.

Injection into the ventricular space is advantageous in that the mechanical deformation produced by the injection is diffusely absorbed. It is then possible to examine the behavioural and electrophysiological effects of such injections on the brain. A most thorough and painstaking symptomology is given by Feldberg with regard to the central action of a variety of compounds. Intraventricular injections of acetylcholine and anticholinesterases produce catatonic stupor. Adrenaline given by the same route produces a sleep-like or anaesthesic effect as opposed to its traditional arousal effects. It is worth noting that anaesthetics such as chloralose also result in an anaesthetic-like condition along with the usual muscular excitability. Furthermore, given intraventricularly, chloralose will produce hyperphagia followed by ataxia and then sleep.

Obviously the ventricular route demonstrates central effects that have previously been obscured due to the blood-brain barrier. What is still not clear, however, is whether this technique will permit a determination of the site of action of these compounds within the brain. To exert an effect the injected compound must first diffuse into and through the brain tissue from the ventricular space. Obviously penetration of the drug into the brain would not be specific to any locus but generalized, being limited only by the surface of the ventricular wall. This problem is considered at length in the book only to indicate that in many cases localization of the general site of action is possible but rather difficult.

A more direct answer to the problem would be, perhaps, the use of stereotaxic placement of compounds in crystalline form directly into the various structures or nuclear centres under investigation. Using this method Grossman has reported adrenergic hunger and cholinergic thirst systems in the ventrolateral hypothalmus, and Hernandez-Peon has also indicated similar systems for sleep in the limbic centres.

For those concerned with neurophysiological and therefore neuropharmacological bases of behaviour, Professor Feldberg's book will be of interest. It is a book which illustrates both the advantages and the disadvantages that are to be gained in the application of a new technique to an old problem.

I. Steele Russell.

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BINOCULAR MASKING INDUCED BY A MOVING OBJECT

BY

G. C. GRINDLEY and VALERIE TOWNSEND

From the Psychological Laboratory, Cambridge University

Movement in a part of either of two binocular fields can, under some conditions, produce temporary obliteration of the corresponding part of the other field. This paper is a mainly qualitative study of this rather surprising phenomenon. The effect is found to increase from the fovea to the periphery, to be greatest at a velocity of about 20° visual angle per sec. and to vary with the orientation of the fixation point in the visual field. Some further lines of research designed to elucidate the relation of the effect described here to certain other visual phenomena are suggested.

INTRODUCTION

In a little known paper by Breese (1899) there is a brief reference to some effects which seem to be related to those described here. He describes for instance how, when looking through a stereoscope, a moving object shown to one eye was dominant for a greater proportion of the time than was a stationary object shown to a roughly corresponding area in the other eye. Breese however, gives little experimental detail.

The experiments described here arose from a casual observation while studying some of the conditions determining binocular fusion and rivalry. It was found that when adjusting the position of a test object in one field of a stereoscope, this movement sometimes caused the momentary disappearance of other objects in the corresponding area of the field of the other eye. Later it was found that a similar effect can be produced by moving an object (e.g. by waving a pencil) over one part of two dissimilar binocular fields; in this case some or all of the second field appeared to be "rubbed out". The experiment was repeated with a number of sophisticated subjects and, although it did not always occur, the effect seemed sufficiently interesting and surprising to warrant further study.

The present paper is a preliminary study of some of the salient features of the effect—using relatively simple displays. The reasons for making this study mainly qualitative are discussed later.

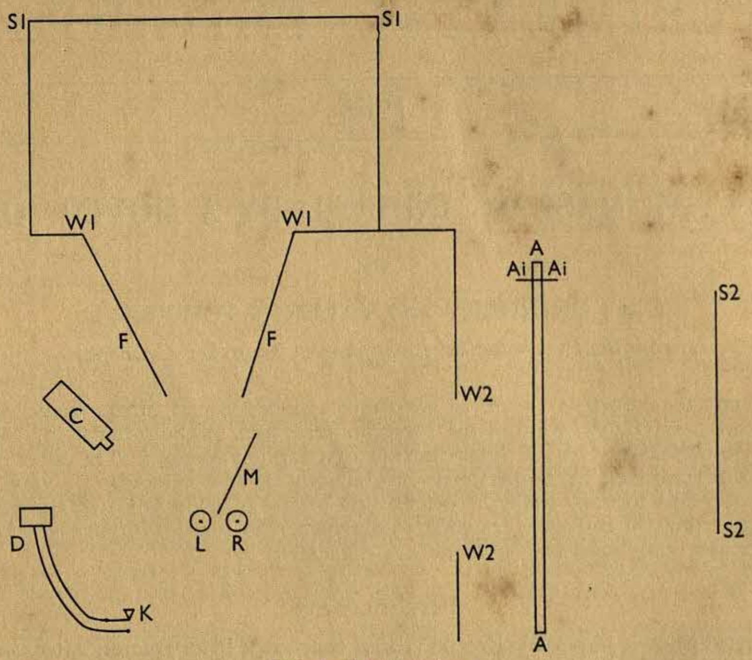
APPARATUS

The apparatus used in most of the experiments is shown diagrammatically in Figure 1.

The subject sat with his chin on a rest and viewed a white cardboard screen (S1S1) with his left eye (L). Movements of this eye could be viewed obliquely through the reflex view-finder of a cine-camera (C) or filmed. Illumination of the eye was provided by two fluorescent lamps (not shown in Figure 1) placed respectively above and below the cardboard funnel (FF). This funnel served to reduce the stray light falling on the screen (S1S1).

The subject's right eye viewed (through a front silvered mirror (M) a second white cardboard screen (S2S2) in front of which was a black wooden arm (AA) mounted on one end of an axle (AiAi). This axle could be driven in either direction by a servo-motor, through reducing gears.

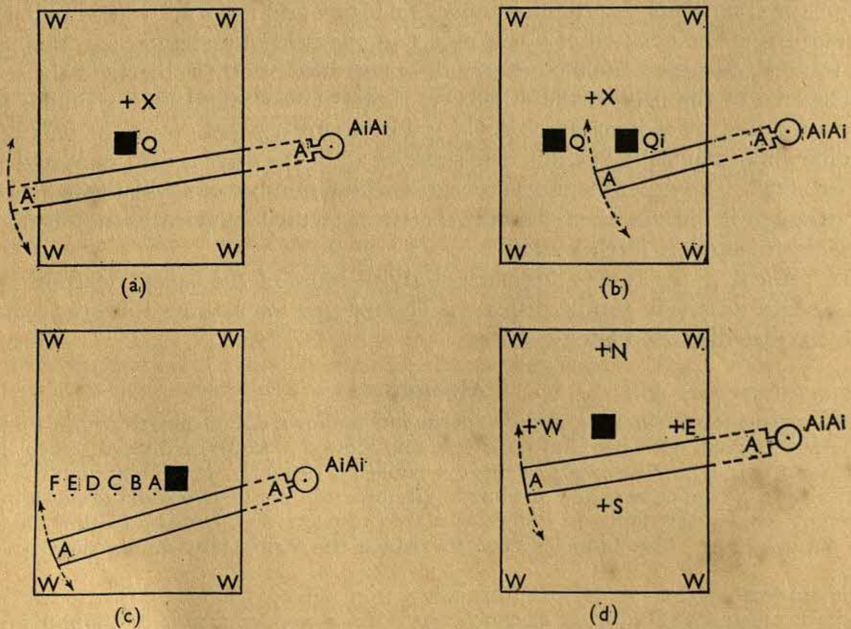
FIGURE 1



Plan of apparatus.

Some of the displays used are shown in Figure 2 (a–d), the visual field is shown by WWWW which represents the combined binocular fields of the two eyes. WWWW is the binocular fusion of the windows W1W1 and W2W2 in Figure 1. The arm was visible to

FIGURE 2



(a) (b) (c) (d)

Displays used in various experiments (2*a*: Experiments 1, 2 and 4), (2*b*: Experiment 1f), (2*c*: Experiment 3), (2*d*: Experiment 5).

the *right* eye only. The rest of the display, i.e. the fixation mark and the square or squares, were visible to the *left* only (except where otherwise stated). The axle A1A1 was not visible to either eye.

A morse key (K) could be operated by the subject's left hand. When this was in use, the subject was told to press the key if and when the square disappeared, and to release it at the moment of re-appearance. The key operated a dekatron timer (D) which was accurate to 1/100 sec.

Dimensions

The optical path from either eye to the corresponding white screen was 30 in. so that ½ in. on either screen subtended a visual angle of about 1°. The width of the arm (AA) subtended an angle of about 2° at the right eye.

The windows W1W1 and W2W2 were at about 16 in. optical distance from the eyes, giving fields of view as shown in Figure 2 of about 29° × 34°. The luminance of the screen S1S1 was about 160 ft. L., and that of S2S2, viewed through the mirror M, about 63 ft. L. The black square Q as seen by the left eye (Figure 2) was 20 ft. L. and the arm, as seen through the mirror, 2·5 ft. L. The illumination of the subject's left eye by the fluorescent lamps was about 20 lumens per sq. ft. This was sufficient to permit cinematography, and as the tubes were about 32° from the line of sight there were few complaints of serious glare. The cine-camera was such that it enabled voluntary eye movements (of the left eye) of 2° to be detected with certainty. (It will be seen later that a much higher sensitivity would have been desirable; since there is some evidence, in Exp. I g, that movements (of the left eye) of as little as 0·5° could affect the results).

EXPERIMENT I

Qualitative description of the phenomenon with very simple displays

With the display shown in Figure 2*a* the subjects were asked to fixate the cross X and report all that they saw. (The black square was of 2° visual angle and at 4° from the fixation mark X, both being visible to the left eye only. The arm crossed the field of the right eye *downwards* at a velocity of about 17° visual angle per second).

The aim was to obtain descriptions of the phenomenon with this simple display from a number of people with varying degrees of experience of visual experiments. There was no set schedule of readings, each subject being allowed to study any aspect of the matter as long as he cared. There were about 50 subjects, varying from school children to retired professors, and including about 20 who had themselves done some research on various problems in the physiology or psychology of vision, as well as a number of undergraduates and casual visitors to the laboratory.

(*a*) *Disappearance of the square.* All subjects reported that when the arm appeared to cross the square the latter *sometimes* disappeared for a time, leaving a completely blank patch. The most usual description was that the square was just "rubbed out" by the arm; and many subjects who had not examined the apparatus beforehand were surprised to find that the arm was really in the field of view of the other eye. Discovery of this fact did not, however, affect the occurrence of the phenomenon.

In some subsidiary experiments the cardboard screens S1 and S2 were interchanged, so that the right eye now saw the moving arm *and* the fixation cross and square, while the left eye saw only a plain white surface. In this case no disappearance of the square was reported—the arm merely "moved over it". This and the results of Experiment 1 (*h*) would suggest that the inhibiting effect of a moving object occurs only in the field of the other eye. There are, however, some interesting observations by MacKay (1960) which show that, under certain conditions, an analogous effect can be produced monocularly. These observations will be commented on in relation to the present experiments in the Discussion.

(*b*) *Onset of the disappearance.* Most subjects said on various occasions that the square disappeared just as the arm crossed it, ("rubbing out"), that it disappeared just before the arm reached it, or that it disappeared just after the arm passed it. (The first was the most frequent account; but most subjects experienced all three phenomena in different trials.)

Several attempts were made by some subjects to say more exactly *when* the square disappeared, usually by estimating the position of the arm at the time, but very few of these reports were made with any confidence. One very experienced subject, (O.L.Z.) who tried this many times said, "it is rather like some of Michotte's experiments: you see that there is a connection, but cannot feel at all sure about the exact sequence".

A few subjects reported on a few occasions that the square, after disappearing, left a negative after-image.

(*c*) *Duration of disappearance.* Most subjects found this very difficult to measure, either on the dekatron or by estimation of the position of the arm. Typical examples of estimations as recorded on the dekatron are given in the results of Experiment 4.

This difficulty in most of the experiments was not only due to the very large inter- or intra-personal differences (which are illustrated in Experiments 3 and 4), but also to the fact that most of the subjects (especially those more experienced in visual research) doubted their own accuracy. In general the subjects said that they were being asked to attend to too many things at the same time. Thus, if asked to fixate a cross, state the moment at which a peripherally viewed square disappeared and also describe its disappearance and re-appearance, the subjects usually said that they could not give an account of this whole sequence of events. Whether a long series of repetition would have improved matters is a complex question, but the usual opinion seemed to be that this led to "parrotlike" repetition—rather than genuine description. In most of our experiments we therefore confined ourselves to the very simple question "did the square disappear?"—and to this most people felt they could give a fairly confident answer.

(*d*) *Partial disappearance.* In all the experiments described the square was sometimes reported to disappear completely but on other occasions some form of partial disappearance was reported.

The following are some descriptions of partial disappearance. The square was often said to get fainter all over, or one side or the top or bottom disappeared leaving the rest intact. These descriptions were usually given with hesitancy, and where we have included them in the results at all (as in Experiment 3) we have classed them together (merely as partial disappearance). To make a list of all the comments and attempted descriptions would be very lengthy, and its scientific value would be highly questionable.

(*e*) *Re-appearance.* This was sometimes said to be very sudden: a fact which might well be due to slight saccadic eye movements (c.f. (*g*) below). More often it was said to be gradual; but the descriptions (often from the same subject) included gradual return of the whole square, or its starting at any one part and spreading until the whole was visible.

(*f*) *Localization of the phenomenon.* In this experiment two squares, side by side as shown in Figure 2*b*, were substituted for the single square shown in Figure 2*a*, and the arm was shortened so that it only appeared to cross one of the squares, and its end just to touch the fixation cross. In this case nearly all subjects reported disappearance only of the right hand square, i.e. the one that appeared to be crossed by the arm. In a few reports of disappearance of both squares, all agreed that the left hand square disappeared much less often and re-appeared much sooner than the right hand square. These few reports can be regarded as the same type of effect as is reported in section "*h*" on the use of more complex figures. All the subjects agreed that the disappearance of the right-hand square was a far more frequent and reliable phenomenon than that of the disappearance of the left.

(*g*) *Effects of eye movements.* All subjects reported that when the arm had caused a disappearance of the square, a slight voluntary eye movement from the centre of the fixation cross to the end of one arm of the fixation cross caused the immediate re-appearance of the square. Such a movement, i.e. about ½° was, unfortunately, too small to be detected with certainty either by the cine-camera or by direct observation; we therefore had to rely on instructions to the subject and on his report that he had carried them out. No one however disputed this effect. A question which arises is whether the great variability of reported times of disappearance as described in later experiments may have been partially due to small involuntary and unconscious eye movements which we were unable to record. In another experiment the subject was asked not to fixate the cross but to follow the arm down through the whole field: this involved an easily observable pursuit movement of 34°. No one reported any disappearance of the square under these conditions. The sides of the window gave a stable frame of reference so that it was quite clear that it was the arm which was seen to be moving across a stationary window. We can therefore attribute the whole phenomenon to the actual movement on the retina rather than to apparent movement in space.

(*h*) *More complex figures.* A few experiments were done using complex displays to both eyes. Some of these were plain or coloured pictures of concrete objects; others were such things as a random collection of squares, circles and triangles or a plainly drawn grid. When the arm moved across the figure shown to the right eye it usually caused some temporary disappearance of part of the figure shown to the other eye. Most often the disappearance effect was more marked behind the trailing edge of the arm than in front of the arm. The phenomenon was complicated, however, by the configurational properties of the field concerned; thus if this contained some rather large pictures of concrete objects it often happened that the whole of one object would disappear as the arm appeared to touch one edge of it.

A very striking effect was found when two such fields were shown and there was no moving arm. In these cases there was usually some combination of fusion and rivalry of the two fields as has often been reported, e.g. by Helmholtz (1924). If one screen was now moved gently through about ½° to 1° this almost always led to temporary but *complete* obliteration of the objects on the other screen.

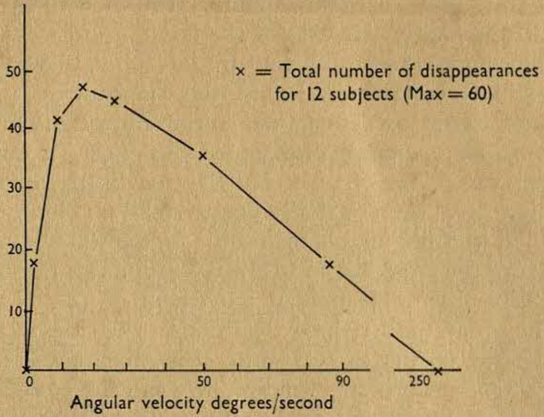
EXPERIMENT 2

Velocity of the moving object

This experiment was intended to show how the speed of the moving object affected the masking. The display to the left eye was a fixation cross at a visual angle of 4°, i.e. 2 in., above the centre of a 2°, i.e. 1 in., black square (as in Figure 2*a*). The black arm was seen by the right eye moving *downwards* at speeds varying from 1·7° to 83·3 degrees/sec. For a higher speed of 250 degrees/sec. a pendulum was used. Twelve subjects did the experiment; they were asked to press a morse key for the duration of the disappearance of the black square and this was recorded on the dekatron. In Figure 3 the curve shows the number of trials on which complete disappearance occurred. It will be seen from Figure 3 that the average frequency of disappearance varied greatly with the speed of the arm, but also it will be seen from Figure 4 that though the frequency of disappearance depended on the velocity of the moving arm, the average *time* of disappearance (when it occurred at all) was not significantly different except at the higher speeds.

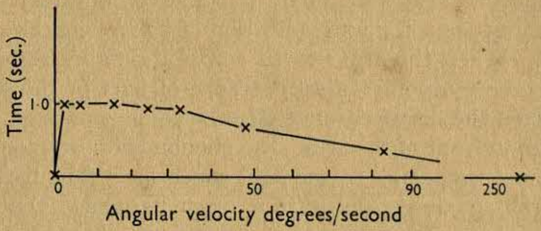
The possible relationship of these results to some recent physiological work on movement will be referred to briefly later (see Discussion).

FIGURE 3



The effect of velocity.

FIGURE 4



Average time of disappearance.

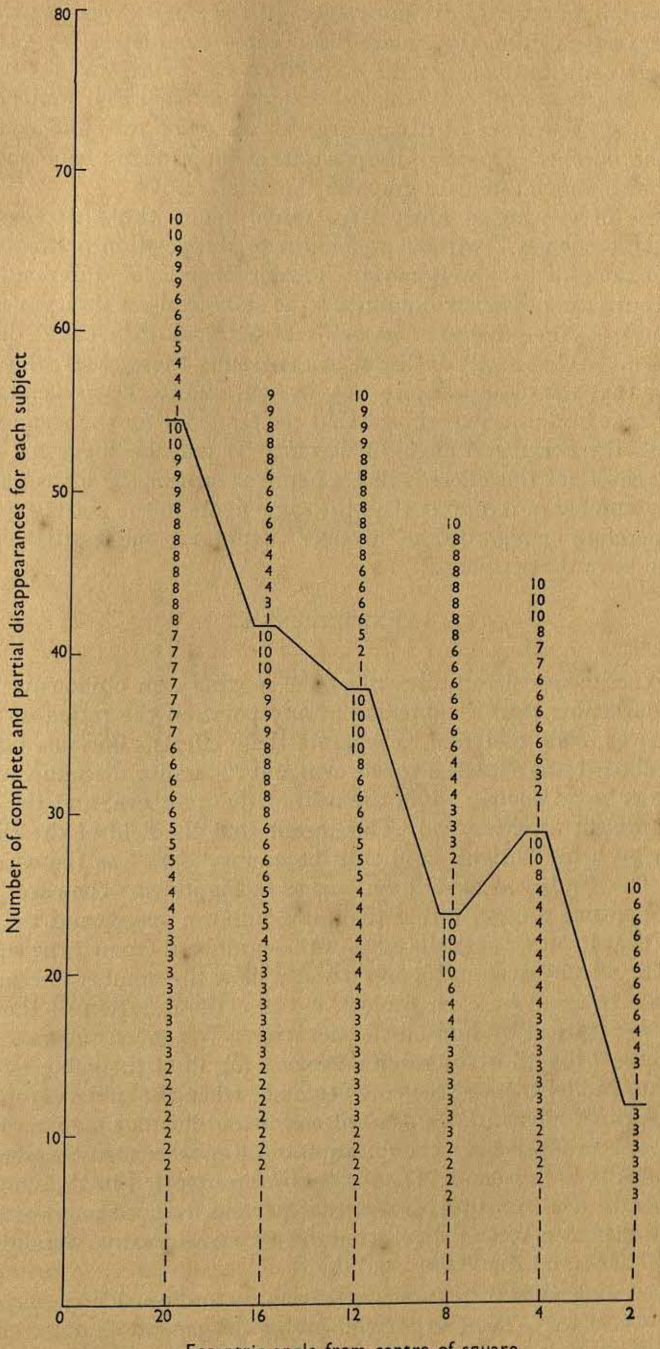
Experiment 3

Peripheral viewing

In ordinary life the appearance of a moving object in the periphery of the field of vision usually attracts attention and causes a movement of the eyes or head to obtain a direct view of the object. This has obvious biological value. But in the present case an attempt was made to prevent any such eye or head movement, and as the moving object was presented to one eye only, it is reasonable to suppose that the main effect was simply the attraction of attention. It is perhaps surprising that this attention should so often obliterate completely the perception of the same area of vision in the other eye; nevertheless, there are plenty of examples where voluntary attention to one (static) part of one visual field temporarily obliterates the corresponding part of the other, e.g. Helmholtz (1924). To describe these phenomena in terms of "attention" does not of course get us much farther in explaining the underlying physiological mechanisms; however it was thought worthwhile to study how the effect varied with the eccentric angle of vision.

The display was the usual 1 in. black square towards the right of the card to avoid the blind spot, together with six fixation crosses at eccentric angles 2°, 4°, 8°, 12°, 16° and 20° from the centre of the square (see Figure 2*c*). The eccentric angle was chosen to give a wide field of view without having the square too close to the boundary of the field. The black arm was seen passing upwards over the fixation points before it reached the square, the time interval being of the order of 0·05 sec. Ten subjects had 8 trials at each fixation point; the results are shown in histogrammic form in

FIGURE 5



Variation of the effect with eccentric angle of vision (each number refers to a particular subject).

Figure 5. Each subject was given a number and the results are plotted so that the individual variations can be clearly seen. The total number of trials and therefore the maximum possible number of disappearances of the square was 80 at each fixation point. At the 20° position the square disappeared completely for 54 of the trials, all the subjects contributing. At the 2° position the figure was 11; with only two subjects reporting the effect. It was also found that when the subjects fixated the square itself, the occurrence of disappearances was very rare and confined to two subjects. The number of partial disappearances reported by each subject at each position is shown above the main graph in the diagram.

In the case of viewing at fairly large visual angles the square sometimes disappeared "spontaneously," without references to the position of the arm. This, of course, is an example of the well-known "Troxler Effect" (cf. Pirenne, 1962), and its occasional occurrence obviously diminished the statistical reliability of the results at the larger angles. Nevertheless most subjects felt that they could distinguish the two effects; and we have only included in the results those cases where the subject felt confident that the disappearance was, in fact, due to the moving arm.

It seems clear from this experiment that the tendency for the square to disappear increases with the peripheral angle of viewing. It may be suggested that part, at least, of the reason for this effect is that a figure at or near the fovea will stimulate a much greater number of receptors than the same figure in the periphery. The central figure can therefore be regarded a "stronger" figure, i.e. one less liable to inhibition than it would be in the periphery.

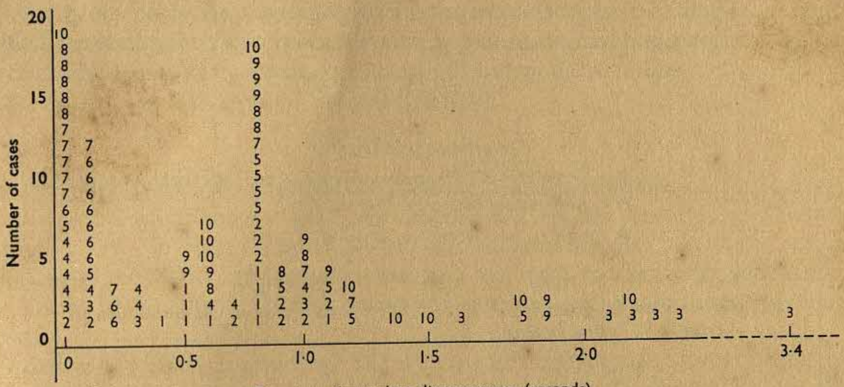
EXPERIMENT 4

On-Off effect

In the experiments already described the moving arm obviously produced successively a darkening then a lightening at any point on the retina of the right eye. An attempt was made therefore to separate these effects. For this purpose a sheet of black cardboard was attached to the arm and the motor disconnected. The black cardboard was broad enough to fill the field of the right eye, and the arm was now driven by a weight at either end. This meant that the field of the right eye could either begin by being "bright" and then becoming "dark" as the cardboard fell or vice versa. The display to the left eye was as in Figure 2*a*. The card, of course, fell with an accelerating motion rather than at a uniform speed, and the weights were arranged so that in either case the edge of the cardboard crossed the square at about 20° a sec. The results given in Figures 6 and 7 show the number of cases of complete disappearance reported by each subject and also the duration of the reported disappearance as recorded by him on the dekatron. Too much reliance should not be placed on these "quantitative" results, because (*a*) all of the subjects doubted their own accuracy, and (*b*) there were many occasions when the subjects reported that the square disappeared momentarily, very often for too short a time for them to operate the morse key. In these cases the duration of disappearance was given the entirely arbitrary value of 1/10 second. The histograms are plotted in the same general way as those given in Experiment 3, except that partial disappearances are not included. Neither inspection nor statistical treatment suggests any significant difference between the "On" and the "Off" condition.

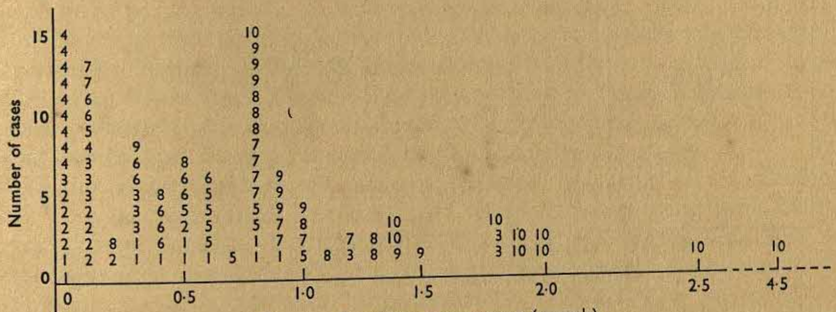
In another experiment the arm was stationary and outside the field of view, and the subject was asked to switch the lights giving the general illumination of the screen S2 on and off at intervals. Very few subjects reported any tendency for the square to disappear under these conditions, though a few said that the square *faded* momentarily when the lights were switched *on*. This experiment confirms the view that the phenomena described in this paper were due to *movement* rather than to changes of illumination in the field of the right eye.

FIGURE 6



"On" Effect.

FIGURE 7



"Off" Effect.

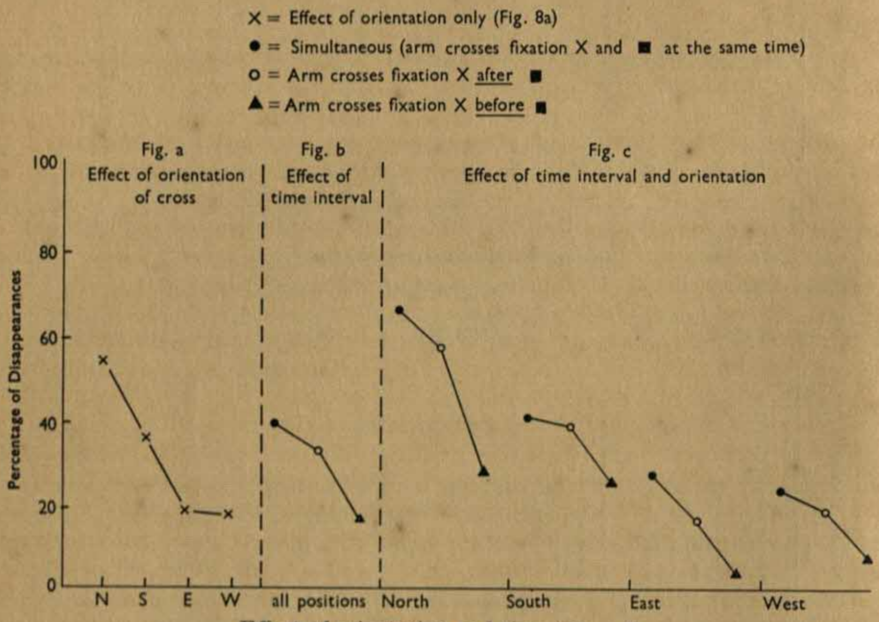
EXPERIMENT 5

Direction of movement and viewing

Some of the preliminary trials described in Experiment 1 suggested that the frequency of the disappearance effects depended not only on the eccentric angle of peripheral viewing but also on the orientation of the square relative to the fixation point. It is not at all clear from previous work on peripheral vision why this should be so. For instance, the decline of monocular acuity as we go out from the fixation point is very similar in any direction (Wertheim, 1894.) In the present experiment, however, we are considering a binocular phenomenon and it is well known to anyone who has used a stereoscope that the tendency towards horizontal convergence or divergence movements is much greater than that towards vertical disparities. It was therefore thought worth while to investigate this point further. The experiment described in this section gives some evidence on the influence, not only of the orientation of the square relative to the fixation cross, but also of the time between the moment at which the moving arm appears to cross the fixation point and the time it appears to cross the square.

Four fixation crosses were placed round the black square at the "North," "South," "East" and "West" positions, each at a 7° visual angle from the square, (cf. Figure 2*d*). The moving arm could go either clockwise or anti-clockwise, and could be pivoted either vertically or horizontally to the square. Thus the arm could cross a fixation point about 0·2 sec. before or after crossing the square, or could cross the square and fixation point simultaneously. It could also cross the square vertically or horizontally. The results are shown in Figure 8 as percentages of the total maximum possible disappearances at each position. Figure 8*a* shows the effect of orientation, of the fixation cross in the visual field, neglecting the direction of movements of the arm. Using Friedman's analysis of variance a X^2 of 12·3 is obtained, which is significant at the 0·01 level. Figures 8*b* and *c* show the effects of the time interval between the arm crossing the fixation point and the square. The position of the fixation cross is ignored in Figure 8*b*, and a X^2 of 9·21 is found, which is also significant at the 0·01 level. It seems clear from these values that both the direction of the fixation point and the relative time of crossing the fixation point and the square had significant effects. Figure 8*c* shows the effect of both the position of the fixation point and the time interval. It will be seen that for all orientations the results go in the same direction, namely that simultaneity causes most disappearances, and that crossing the fixation point *after* crossing the square gives more disappearances than crossing the fixation point *before* the square.

FIGURE 8



Effect of orientation and time interval.

EXPERIMENT 6

Reversal of figure and ground

In all the experiments so far described the display has been a black arm passing over a white background with a black square and fixation point. In order to see if the masking effect was dependent in any way upon this particular display, another experiment was carried out with the figure and ground reversed. A white arm was seen by the right eye passing over a black ground, while the left eye saw a white square and fixation point, the point being 7° visual angle above the square. Only three subjects did the experiment, but the effect was so striking that it seemed unnecessary to continue. Masking of the square occurred in *every* trial, and it is therefore obvious that the phenomenon is not dependent upon the sign of the luminance difference of figure and ground. In this experiment the luminances of both the square and the arm were of the order of 7 ft. L.

DISCUSSION

In this paper we have examined only a few of the determinants of the phenomenon described; this phenomenon will now be referred to, for brevity, as "movement masking." It is obvious that further experiments are needed, not only to examine other determinants of "movement masking," but also to clarify its relationship to other visual phenomena. In this discussion several such possible lines of work are suggested.

The effect of voluntary attention. We are now attempting some experiments on the effect of voluntary attention on "movement masking." It is well known that in monocular tachistoscopic experiments voluntary attention to one part of the visual field can, *without eye movements*, greatly increase the clarity with which objects in that region are seen (cf. Helmholtz 1924, Grindley 1931, and more convincingly, Averbach and Sperling 1960). In binocular vision Helmholtz (1924) and many later observers have claimed that it is possible to make one of two rivalling figures dominant for a considerable time. What we are now concerned with is whether voluntary attention to an object, such as one of the squares used in the experiments described here, can influence the frequency or duration of its disappearances in "movement masking" conditions. We hope to report on this in a later paper.

Monocular rivalry. There are some interesting observations on "monocular 'rivalry'" by MacKay (1960) to which brief reference has already been made (see Expt. 1*a*, above). MacKay found that if he viewed in silhouette a thin wire whose diameter subtended an angle of about 10 min., and whose retinal image was stabilized by a simple lens device, the wire "often remains visible for some time". Whether this "reluctance of the wire . . . to disappear" is due to imperfect stabilization he leaves an open question, though he suggests other possible interpretations. (His degree of stabilization would in any case be much greater than ours, since we relied only on voluntary fixation.) MacKay continues:

> "If, however, an object giving an unstabilized image, such as the finger-tip, is now brought up just behind the wire to the focal zone of the lens, a striking change takes place. The stabilized wire disappears as a whole, and the finger-tip appears completely visible, as if nothing were between it and the eye. The impression is similar to that given by binocular rivalry between disparate images in left and right eyes. When the finger is removed the wire re-appears."

Thus MacKay's effect is in many ways a monocular analogue of what we have called "movement masking," and we are indebted to him for drawing our attention to this. It may well be that our failure to obtain a monocular disappearance effect (in Expt. 1*a*) was due to imperfect fixation, but if this is so, it is rather puzzling that we should obtain a binocular effect. Perhaps the relationship between all these phenomena could be explored further by devising an apparatus in which a stabilized image of a fairly small object would be presented to one eye, while in an area closely adjacent to this in the visual field a moving object or a moving pattern could be presented to either eye.

Binocular meta-contrast and rivalry. Two other phenomena which invite comparison with "movement masking" are those of binocular meta-contrast and those of binocular rivalry. The phenomena of meta-contrast have been studied extensively since the early work of Werner (1935) (1940). Essentially the problem is that of the conditions under which the perception of a form exposed tachistoscopically can be inhibited by the tachistoscopic presentation of another form either shortly before or shortly afterwards. The effect can be obtained even if the two forms are presented to different eyes (binocular meta-contrast). The conditions governing the phenomenon, and determining whether "fore-masking" or "after-masking" will occur are too complex to be described here. The duration of the exposures and the time interval between them, the luminances and contrast values, the state of adaptation of the eye, and the degree of congruence of contours between the two forms, are important factors. A paper by Kolers and Rosner (1960) describes some of their own experiments and also summarizes other work on binocular meta-contrast up to that date.

Binocular rivalry is a well-known phenomenon about which there is a considerable literature. If the displays to the two eyes are both very complex and entirely different (e.g. a city scene to one eye and a country scene to the other) the resulting binocular perception is usually one of ever changing confusion, from which it is very difficult to infer any operative principles. If, on the other hand, the displays are made simple it becomes easier to make empirical generalizations. Whittle (1963) has found, *inter alia*, that for very simple geometrical achromatic figures the rate of rivalry is very much greater if the contours of the two are exactly congruent than if they are not. He also found that the tendency of a figure to be visible for a greater proportion of the time than the other figure will increase both with the absolute luminances of the figure and its ground, and also with the contrast, i.e. the ratio of these luminances. This is true over a very wide range of photopic luminances.

We can thus see that at least three factors, luminance, contrast and congruence of contours, are important both in meta-contrast and also in rivalry. To test the importance of the first two in "movement masking" would be a straightforward, though somewhat lengthy task. The third, congruence of contours, has no obvious parellel in "movement masking" but suggests that it would be important to test the localization of the effect more accurately than we have done in Expt. 1*f*.

Movement perception. The relationship of "movement masking" to the perception of movement (whether real or apparent) is not yet clear; and anything that can be said about the underlying physiological mechanism must, at present, be highly speculative. On the first of these points it will be seen from Expt. 1*g* (in which the subject's eyes followed the moving arm downwards) that the disappearance of the square seemed to depend on actual movement on the retina rather than on apparent movement in space. It would be interesting to find whether other types of *apparent* movement, e.g. the phi-phenomenon, *can* produce the disappearance effect. On the second point, it is worth noting that cells which respond selectively to movement, and sometimes to the direction of the movement (without responding to changes in illumination of the area concerned) have recently been discovered at various levels in the visual system of mammals. Thus Hubel and Wiesel (1959) found such cells in the striate cortex of the cat, Barlow, Hill and Levick (1964) found that some of the ganglion cells of the rabbit's retina were similarly selective, and Hill and Horn (1964) found similar cells in the superior colliculus of the rabbit. Though there is no direct evidence of such cells in the human visual system, there is a good deal of subjective evidence (e.g. in the after effect of seen movement) that, *somewhere* in the system, there is a mechanism which can give apparent movement and which does not depend on a succession of "on" and "off" effects in the retina. *If* there is such a mechanism, its role in what we have called "movement masking" is not proved by the experiments described in this paper. There is, however, one interesting feature of the physiological results which may *possibly* be relevant. While Barlow, Hill and Levick (1964) found that "movement-selective" ganglion cells in the rabbit's retina respond maximally to velocities of only a fraction of 1° visual angle per second, Hill and Horn found a maximum disturbance in the superior colliculus at velocities of the order of 20° per sec., which is of the same order as the optimum velocity for "movement masking" under the conditions described.

We wish to thank Professor Zangwill for his constant encouragement and also to thank him and several colleagues, especially Drs. S. M. Anstis, A. Cowey, W. A. H. Rushton and H. G. B. Thomas, for the time they have spent as subjects, and their many suggestions. We are also indebted to the Medical Research Council for a personal grant to one of us (V.T.) and an apparatus grant.

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AN INVESTIGATION OF VIGILANCE IN THE RHESUS MONKEY

BY

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From the University of Maryland

The experiments reported here investigated the effects of immediate reinforcement contingent upon detection of a signal in a vigilance task.

Three Rhesus monkeys were trained to report the presence of a 2-sec. signal over a 4-hr. watch. They received a food pellet for each randomly occurring signal detected.

The results indicated that (1) variation in signal rates did not produce differences in detection, (2) no difference in detection occurred over hours of watch, (3) detection and deprivation level were linearly related ($Y = 4{\cdot}1408X + 3{\cdot}9900$), (4) low motivation (8-hr. deprivation) performance was lower ($f < 0{\cdot}01$) than high motivation (24-hr. deprivation) for comparable signal frequencies, and (5) the temporal course of a 1·78 mg./kg. dosage of chloropromazine and detection efficiency was found to be logarithmic ($Y = 78{\cdot}33 \log X - 19{\cdot}08$).

INTRODUCTION

The purpose of the present series of studies was to investigate the effects of immediate reinforcement contingent upon detection of a signal in a vigilance task.

Recent studies (Adams, 1956; Bakan, 1955; Bartlett, Beinert and Graham, 1955; Carpenter, 1948; Deese and Ormond, 1953; Holland, 1958; Jerison, 1958, Mackworth, 1950; Whittenburg, Ross and Andrews, 1956) have shown that curves describing the performance of groups of human subjects indicate that detection efficiency decreases as the time of watch progresses (McGrath, Harabedean and Buckner, 1961). Efficiency usually reaches a minimum within the first hour of watch after which performance remains stabilized at a low level of efficiency. There have been observations of a slight recovery of performance efficiency near the end of the watch period (Mackworth, 1948; 1950). In most cases, an increase in latency of response has accompanied the decrement in detection performance. The percentage of signals detected has been demonstrated to be positively related to the rate of signal occurrence (Jenkins, 1958; Deese and Ormond, 1953).

PROBLEM

A major impetus to the conduct of these experiments derived from the desire to investigate the role of motivation in detection performance. Another major impetus to the conduct of these experiments derived from the fact that no good laboratory model was available for studying vigilance utilizing animal subjects.

Concern was with a situation in which an animal had been instructed, through discrimination training, to report the presence of some specific change in its operating environment. This change was designated as the signal to be detected. The conditions investigated were as follows: (1) detection efficiency as a function of signal rate and duration of the task, (2) the functional relationship between signal detection and motivation, (3) the interaction of motivational level and signal rate, (4) the effect of irrelevant stimuli on detection, and (5) the effect of chloropromazine hydrochloride on detection level and the temporal course of the chloropromazine hydrochloride effect.

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METHODS AND PROCEDURES

Three female Rhesus monkeys served as subjects in these experiments. These animals had no previous training in the experimental task. Their ages were 36, 44, and 46 months, respectively. All three animals were used in all experiments except the second part of Experiment 5. Only two were used to investigate detection level and the temporal course of chloropromazine. After the behaviour of the subject had indicated that the desired level of discrimination had been attained, each subject was tested for four hour periods. Care was also taken to ascertain that the subjects' "false alarm" rate was low and constant. All animals received extensive pre-experimental training on all rates of signal presentation. It was necessary to design a special contingency, however, for *one* animal in order to reduce his "false alarm" rate. A contingency was designed whereby the experimenter could cause the onset of the signal to be delayed by any desired duration from 1 sec. to 60 min. A delay interval of 5 min. was used during this pre-experimental training until "false alarm" rate reached the defined low level. This special contingency was not used during the experiment proper.

APPARATUS

A visual display apparatus was designed on which a continuous (once per sec.) flashing stimulus (red light) was provided by a Foringer Flasher. The signal to which the subjects were trained to respond was a green light (2 sec. duration) randomly presented on the same display as the continuously flashing red light by a Foringer Film Timer and its related Foringer component, the Interval Programmer Panel. The rate of presentation of the signal was controlled by the frequency of holes punched in the tape driven by the Film Timer. The lights were displayed on a 7 × 5 in. milk glass plate illuminated from behind. No efforts were made to equate the brightnesses of the background (continuously flashing red light) and signal lights. Both lights were produced by 25-watt bulbs. The major concern was that the subjects could clearly discriminate between the two stimuli as evidenced by their discriminatory responses during pre-experimental training. Overhead lighting was provided by a 25-watt white bulb throughout all experimental sessions.

The subjects were tested one at a time and were placed in front of the display at a distance which enabled manipulation of the bar, (5½ in. long, ½ in. wide, ⅛ in. thick, with a knob ¾ in. in diameter screwed on the end; force required to depress the bar was approximately 20–25 gm.). Food (monkey banana pellets, 1 gm. each) was obtained from a food tray mounted on the display apparatus and delivered by a Foringer Feeder. A water bottle was attached to the apparatus so that subjects could obtain water while viewing the display.

The animals were seated on a Findley Freestone Cart and restrained by a Findley Freestone Harness. The display and the Findley Freestone Cart were attached to the floor to prevent the animals from moving them.

TASK

The subject was trained to report the presence of the green light (signal) by pressing the bar in its presence. This training was accomplished by the operation of differential reinforcement. After discrimination training, various random schedules of signals presentation were arranged. Only those bar presses made during the presence of the green light produced reinforcements (food pellets). Only one pellet per signal detected was given.

It should be noted that, throughout all of these experiments, performance has been measured by percentage of signals detected. The animals did respond in some cases when no signal was present; however, they were not reinforced for these "false positive" responses. Pre-experimental data (training data) indicates that the "false positive" responses are a function of deprivation level, training level, time at the experimental task. "False positive" responses increase with deprivation, decrease with training time, and decrease with time at the experimental task. The mean "false positive" response for the first hour was 10·48 averaged over the three subjects, with SD = 2·36. The mean "false positive" responses for hours 2–4 was 3·56 with SD = 1·42. There was no evidence that "false positive" responses were a function of signal frequency. "False positive" responses remained low and constant for the signal frequency conditions used in this study.

EXPERIMENT 1: SIGNAL RATE AND DURATION OF WATCH

Procedure

In this experiment, the animals were tested after 24 hr. of food deprivation. The frequency of occurrence of signals varied in the following manner: 20, 14, 10, 8, 6, and 2 per hr. The duration of the watch session was 4 hr. A correct response (lever press) in the presence of the signal delivered a food pellet to the monkey.

The frequency condition used each day was selected randomly with the exceptions that adjacent frequencies were separated by at least 1 day, and alterations between the high frequencies (20, 14, 10) and the low frequencies (8, 6, 4 and 2) occurred at least 1 out of 3 days. Three sessions at each signal frequency condition were recorded for each of the three subjects.

Results

Friedman's two-way analysis of variance of the percentage of detection was computed for each hour of watch (subjects x frequency of signal). The results of this analysis indicated that percentage of detection did not differ for the different frequencies during any hour, or when all 4 hr. were combined. They were all between 93 and 100 per cent. Friedman's two-way analysis of variance for "time of watch conditions" with "frequency" held constant indicated that the percentage of detection was not significantly different for any of the four hours, being 97·00, 97·80, 96·00, and 97·00 respectively for hours 1–4.

EXPERIMENT 2: DETECTION EFFICIENCY AND MOTIVATION LEVEL

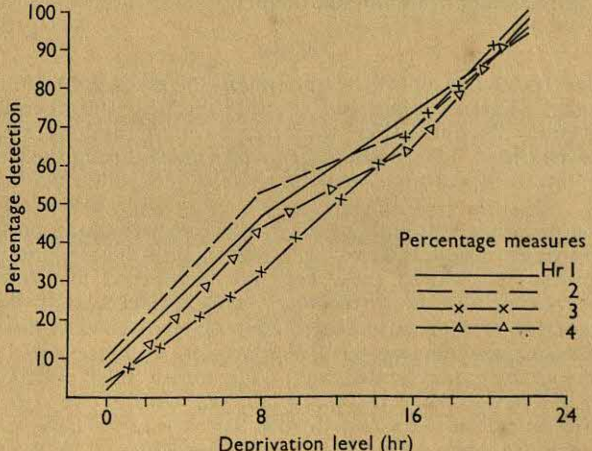
This experiment involved the operation of deprivation-satiation procedures as they relate to signal detection. Here the functional relationship between detection efficiency and motivation level was investigated. Motivation level is operationally defined in terms of length of deprivation, or the time since satiation occurred. The apparatus, duration of signal and performance measure were the same as those described for Experiment 1. Ten signals per hr. was used as the frequency condition.

Deprivation level was varied so that the animal would start on the vigilance task at the following times after having been allowed to eat the amount of food needed to reach satiation: 0, 8, 16, and 24 hr. after satiation.

Results

Figure 1 shows the percentage of detection for each deprivation level. The parameter of the graph is hour of watch, and each point represents the average

FIGURE 1



Deprivation level and detection.

detection level for three animals. The Alexander trend test was utilized to test the hypothesis that the slope of the line was significantly different from zero and that the deviation of the function (for all subjects) from linearity was not significant. An angular transformation of the percentage measures was obtained for this analysis.

The relationship between detection efficiency and deprivation level was shown to be of the form $Y = aX + b$. A "least square" fit of the function yielded the equation: $Y = 4{\cdot}1408\ X + 3{\cdot}9900$. This function was obtained from the data averaged over the 4-hr. period.

The relationship was satisfactorily described as linear and Alexander's trend test for *each* hour separately and all hours combined led to the same conclusion. There were no consistent fluctuations apart from a linear trend. The F ratios derived from the group variance of the slope over individual deviations from estimation were all significant at the 0·01 level of confidence.

EXPERIMENT 3: THE INTERACTION OF MOTIVATION LEVEL AND RATE OF SIGNAL OCCURRENCE

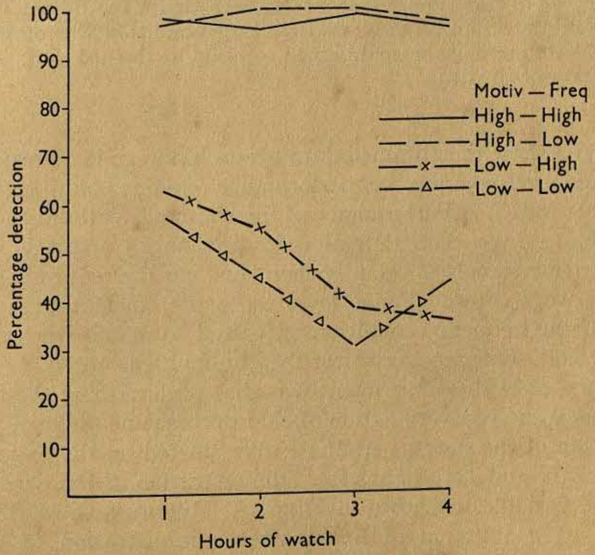
Procedure

This experiment involved running subjects under 24 and 8 hr. of food deprivation with 20 and 4 signals per hr. All subjects were given three sessions with the 24-hr. deprivation condition for signal frequency conditions of 20 and 4 per hr. and three sessions with the 8-hr. deprivation level for 20 and 4 signals per hr.

Results

The Wilcoxon Matched Pairs Signed Ranks test indicated that the "high motivation-high frequency" condition did not differ from the "high motivation-low frequency" frequency condition. Comparison of "high motivation-high frequency" with "low motivation-high frequency" showed that performance was poorer under the latter condition. The Wilcoxon Matched Pairs Signed Ranks test gave a p value less than 0·01 for this difference. Comparison of the "high motivation-low frequency" conditions with "low motivation-low frequency" indicated that the difference between these conditions was significant. The low motivation conditions do not differ significantly from one another. These results are shown in Figure 2.

FIGURE 2



Motivation and signal rate.

EXPERIMENT 4: DETECTION AND IRRELEVANT STIMULI

Procedure

This experiment was designed to determine if level of performance would remain invariant as the number of irrelevant stimuli that is discriminable from the signal. This relationship was investigated at the deprivation level which produced maximum detection efficiency.

A slight modification was made in the apparatus in this experiment. To introduce "noise," two more continuously flashing lights were added to the display. This was accomplished by adding two more flashers to the instrumentation panel. This made it possible to display either a red, blue or yellow continuously flashing light, any two combinations of the three or all three simultaneously.

Signal frequency conditions employed consisted of 20, 10 and 4 per hr. In essence, this was represented by a 3 × 3 design with three irrelevant stimulus conditions and three signal frequency conditions.

Results

Friedman's two-way analysis of variance for "conditions" (one, two and three irrelevant stimuli) and "subjects" indicated that the irrelevant stimulus conditions had no effect on the performance of subjects in this task for hours 1 through 4. Performance for each of the four hours of the three stimulus conditions ranged between 90–100 per cent.

EXPERIMENT 5: THE EFFECT OF CHLOROPROMAZINE ON DETECTION

Procedure

This experiment was designed to test the effects of chloropromazine hydrochloride on detection efficiency. The unique pharmacological action of this drug has been represented as a selective inhibition of subortical centres of the brain, the reticular system, thalamus, hypothalamus, and the antonomic nervous system, without significant effect upon the cortex, although alertness and spontaneity are reduced (Welsh, 1958).

All subjects were given saline injections before administering the chloropromazine. The chloropromazine dosages were administered immediately prior to the experimental session. All injections were given intramuscularly. During the experiment proper, alternations on a day-to-day basis were scheduled for drug-saline conditions.

The dosage levels used were 3·57 mg./kg. and 1·78 mg./kg. Signal frequency conditions used were 20, 10, and 4 per hr.

A second purpose of this experiment was to investigate the duration of the effects of chloropromazine on detection. Only one frequency condition was employed, that of 10 signals per hr. Watch sessions were designed to begin at the 3rd, 6th, 9th and 12th hour after administration of the drug.

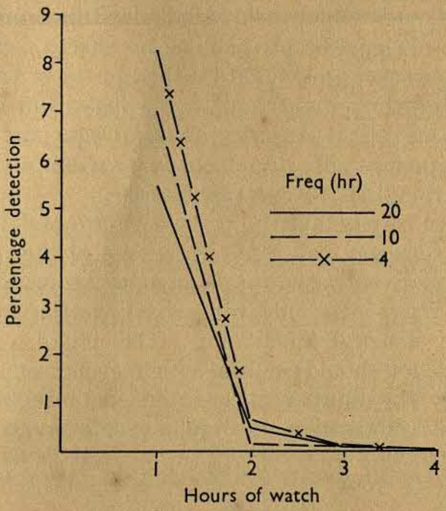
Results

Figures 3 and 4 show the obtained data for each dosage level respectively. With the higher dosage level 3·57 mg./kg. performance remains below 10 per cent. for the last three hours of watch. (Performance of these animals without the drug was equal to or greater than 90 per cent. detection for any hour.) At this dosage level the animal allowed many signals to occur without making the required learned response.

Performance with a lower dosage level was more efficient than with the higher dosage level. Using frequency conditions 4, 10 and 20 signals per hr. with the dosage level at 1·78 mg./kg., some recovery after the 3rd hour was noted.

A second purpose of this experiment was that of describing the function relating detection efficiency and recovery action of chloropromazine (defined as the time since the administration of the drug). Animals were started on the experimental session at the 3rd, 6th, 9th, and 12th hour after administration of the drug. The function relating to these variables is shown in Figure 5. Figure 5 shows the arc sin of per cent. detection as a function of hours after administration of chloropromazine hydrochloride. The equation of this line is $Y = 78{\cdot}33 \log X - 19{\cdot}08$.

FIGURE 3



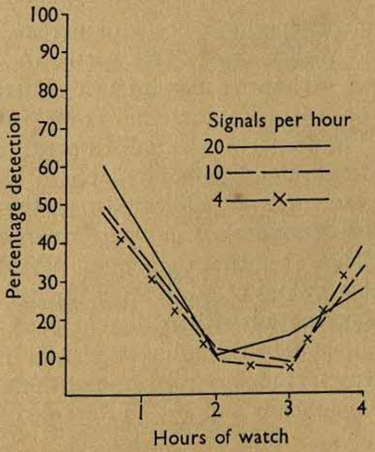
Chloropromazine (3·57 mg./kg. dosage).

DISCUSSION

The experiments described above were concerned with the influence of motive-incentive conditions on vigilance performance. No systematic or thorough investigation of this variable has been noted. There has been some indication that both monetary reward for good performance and at least one form of punishment for poor performance are effective in improving the probability of detection (Pollack and Knaff, 1958). Mackworth (1950) found that providing the observer with immediate knowledge of the results of his vigil was helpful in reducing the decline in performance as time of watch progresses.

The results of Experiment 1, which investigated vigilance as a function of how often the signal occurred, differ from the results of previous investigations. Most investigators have found a decrement in performance for infrequently occurring

FIGURE 4



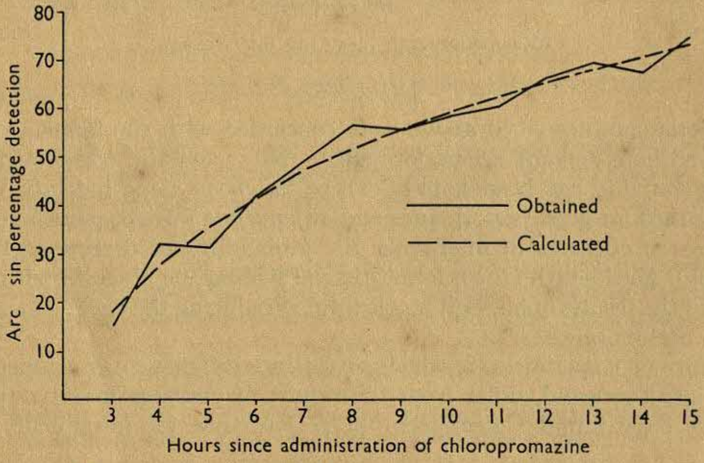
Chloropromazine (1·78 mg./kg. dosage).

signals. The result of this experiment showed that when task-specific motive-incentive conditions are operationally held high and within limits constant throughout the watch session, rate of signal occurrence had no effect on performance. Jenkins (1958) found that performance (measured by the arc sin of the mean percentage of detections) was more nearly proportional to the logarithm of signal rate, that is performance decreases as signal rate decreases. Deese and Ormond (1953) also demonstrated that the performance decrement in a vigilance task was less with high rates of signal presentation than it was for low rates.

It appears that when care is taken to make each signal detection produce a reinforcement, maximum efficiency will result regardless of the number of signals presented. It should be relatively easy to train an animal to watch a display for as long as he is physically able if this is the only way he has of getting food or other substances needed for proper physiological functioning. The use of a partial reinforcement schedule for signal detection should result in a high level of signal detection.

In the present study, the duration of the intersignal interval did not appear to be a critical variable when motivation was high. Some investigators have reported either an increase or no change in performance with increase in the interval (Deese and Ormond, 1953; Jerison and Wallis, 1957; Mackworth, 1950; and McCormack, 1958) or a decrease in performance with increase in interval (Bartlett, Beinert and Graham, 1955; Jenkins, 1958; Jerison, 1959; Kappauf and Powe, 1959).

FIGURE 5



Temporal effects of chloropromazine on detection.

If motivation is indeed an important variable operating in vigilance tasks, then the observed performance level should be manipulable by variations in the motivation level. The results of the present study indicated that the function relating motivation (measure by hours of deprivation) and performance (percent detection) was linear. It should be noted, however, that the longest period of deprivation was 24 hr. It is possible that for higher values of motivation (deprivation greater than 24 hr.) one might find another linear function with a different slope, or a positively accelerated function. Future research should utilize longer periods of deprivation.

The data showed no interaction between motivational level and rate of signal occurrence. This finding also lends support to the role of motivation (deprivation) as a major operating variable.

Results of Experiment 5, concerned with the depressant effects of chloropromazine upon signal detection performance, call attention to the participating role of selected nervous system processes in the mediation of such vigilance behaviour. These results suggest, for example, the inhibition of the arousal system (midbrain reticular formation) may greatly disturb performance in a task calling for a high degree of attention. It is also possible that among the many CNS effects of a drug like chloropromazine, specific actions may occur in the thalamus, the relay station of the brain. The inhibition of this system might decrease the efficiency of the organism in performing learned functions, due to ineffective relaying of information.

Finally, it is conceivable that chloropromazine effects upon the hypothalamus, involving appetite suppressive systems, could have reduced motivation and possibly account for the decrement in detection performance. Studies based on aversive rather than positive reinforcement are required to settle questions of the best characterization of the drug effects.

This paper is based upon a dissertation submitted in partial fulfilment of the requirements for the Ph.D. degree at the University of Maryland. This investigation was supported by Research Grant MU-1604 from the National Institutes of Mental Health, United States Public Health Service. The author is indebted to Nancy Anderson, T. G. Andrews, and J. V. Brady for their enthusiastic advice and encouragement.

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THE BENEFICIAL EFFECTS OF MEPROBAMATE ON DELAYED RESPONSE PERFORMANCE IN THE FRONTAL MONKEY

BY

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Previous studies of antagonistic treatments on delayed response performance by frontal monkeys suffer from a logical flaw in that the treatments may also improve the performance of normal monkeys. In a previous study (Gross and Weiskrantz, 1961) we have shown that meprobamate is without effect on delayed response in normal monkeys, but it does severely depress their discrimination performance. In the current study meprobamate is shown to produce a significant improvement in delayed response in each of three frontal monkeys. Nembutal is also effective. Various interpretations of the results are discussed, the view being favoured that the frontal monkey suffers from an excessive and inappropriately ordered intake of sensory information.

It is tempting to try to gain an understanding of pathological states by identifying their antagonists. In this connection attempts have been made to alleviate the well-known delayed response impairment produced by dorsolateral frontal cortex lesions in the monkey by a variety of treatments, among them pre-delay reward (Finan, 1942), darkness during delay (Malmo, 1942; Orbach and Fischer, 1959), food deprivation (Pribram, 1950; Gross, 1963*b*) and various drugs (Wade, 1947; Pribram, 1950; Blum, Chow and Blum, 1951; Mishkin, Rosvold and Pribram, 1953; Riopelle and Pfeiffer, 1958; Dean and Davis, 1959). From these studies have come various characterizations of the frontal defect, viz., that it in each case shows features opposite from those thought to be associated with each treatment. There is a logical difficulty associated with all of these attempts (also discussed elsewhere, Gross and Weiskrantz, 1964) of the following nature: a treatment may appear to be antagonistic whereas it is having its effect merely by making the situation more easily solved by all subjects irrespective of whether or not they show pathology. For example a person with poor visual acuity may nevertheless improve his performance on a visual task if distracting auditory noise is reduced, or if he is rewarded handsomely for his visual performance. It would be incorrect to conclude that the defect is one either of hyperdistractability or weak motivation. The question can be sorted out by determining whether the same treatments beneficially affect individuals without pathology. Unfortunately many of the studies purporting to demonstrate antagonistic treatments of the frontal defect fail to include any normal animals. Even when they are included, however, they may be performing already at their upper asymptote and so no improvement would be expected.

The current study attempts to study the effects on frontal monkeys of a drug treatment which is known to be without reliable effect on delayed response performance by normal monkeys (Gross and Weiskrantz, 1961). The normals had been deliberately kept at sub-asymptotic levels of performance by continuously varying the difficulty of the task. In the previous study both meprobamate and reserpine were employed. In the current study only meprobamate was used, and also sodium pentobarbitone (Nembutal) for comparative purposes.

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** Dr. Baltzer participated in this work when he was on leave of absence from the Department of Pharmacology, Ciba, Basle, Switzerland.

METHOD

Subjects

Three rhesus monkeys were used. Two of them (Nos. 8 and 9), weighing 4·25 kgm each, had received dorsolateral frontal cortex lesions 18 months prior to these tests, and had been involved in an earlier study (Oxbury and Weiskrantz, 1962) on learning set, during which it was shown that they could not solve a 5 sec. delayed response problem. The third monkey (No. 10F), weighing 6 kgm, had also been operated on approximately 18 months prior to these tests. It had a mixed post-operative behavioural history spent largely on a time-discrimination test, and saccharine preference testing, but not on delayed response. None of the animals had received pre-operative delayed response training. The lesions of these animals have not yet been reconstructed but were intended to be the same as those made in a large number of studies, e.g. group T in Gross (1963*a*).

Delayed Response Test

Testing was carried out in a Wisconsin General Test Apparatus. Two identical plaques covered two food wells located 12 in. apart. At the start of each trial the plaques covered both food wells. When the opaque screen was raised the experimenter showed the monkey a peanut for 5 sec., displaced one plaque and placed the food in the well under it, replaced the plaque and then lowered the opaque screen for a specified delay interval. Then the screen was raised and the animal permitted to displace one of the plaques and possibly obtain food. The position of cup baited was varied from trial to trial according to a random table. No correction trials were given. Fifty trials were given in each session.

As will be seen, with one animal it was necessary to use a transparent glass screen rather than an opaque screen during the interval between baiting and response.

Drug and Testing Schedule

Monkeys No. 8 and No. 9 had a schedule of testing with drug injection prior to the start of the main part of the experiment. Both animals were first tested with meprobamate four sessions (with a rest day between sessions), using an intra-trial delay of five sec. It was hoped that several drug sessions given in succession might permit the animals to learn to perform delayed response at a higher level of accuracy than would be possible with single sessions alternated with control sessions. In the case of one animal, No. 8, this seemed to have happened, since it averaged 83 per cent on the final two days of testing. Subsequently it performed at 88 per cent with the drug, as contrasted with 70 per cent and 68 per cent with saline on the session just prior to and following the drug session. In the case of No. 9, however, five sec. appeared to be too long a delay even with the drug, and performance remained about chance level with the drug and subsequently with saline.

It was decided, therefore, to make the problem simpler by decreasing the delay to one sec. for both animals. Even this, however, proved too difficult for No. 9 and various other stratagems were tried, such as moving the plaques towards the animal before lowering the screen, and finally a successful arrangement was achieved by using a transparent glass screen rather than an opaque screen during the delay period, with a delay period of 2 sec. No. 8 was continued with one sec. delay using the opaque screen.

Once the appropriate level of difficulty had been found, these two animals were tested with intramuscular meprobamate, 125 mgm/kgm, with polyethylene glycol 400 as the vehicle, or, as a control, with an equivalent volume of isotonic saline. Testing was carried out every other day, and drug and saline sessions were alternated. Control injections of the vehicle were also given. Testing was carried out 1½ hours following injection. Finally intramuscular sodium pentobarbitone (Nembutal) was given, 15 mgm/kgm, which is approximately half the anaesthetic dose, and, on one occasion, an identical dose of methylphenidate (Ritalin). This dose was based on earlier work from this laboratory in which Ritalin was shown to affect behaviour of normal monkeys in a timing situation. The post-injection was one hour for Nembutal and Ritalin.

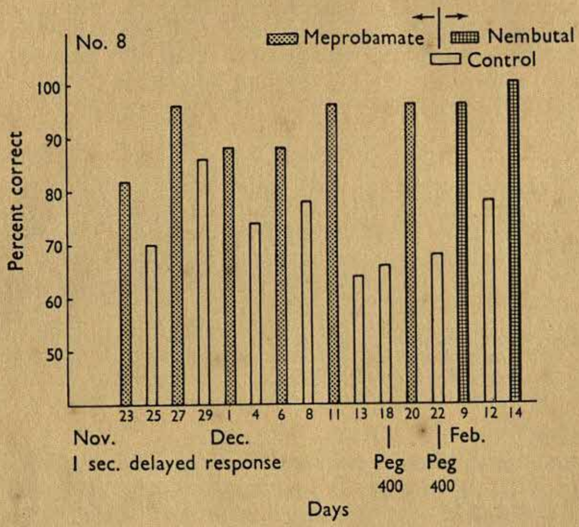
Monkey No. 10F was tested on an intra-trial delay of one sec., using the opaque screen. It was injected with 150 mgm/kgm meprobamate or an equivalent volume of isotonic saline intramuscularly. Drug and saline control sessions were alternated for a total of 10 sessions. The interval between injection and testing was always two hours. The slightly lower dose of meprobamate for animals 8 and 9 was chosen because 150 mgm/kgm in preliminary testing produced slight muscular weakness in these two animals.

The precise testing schedule can be seen in Figures 1, 2 and 3.

RESULTS

As can be seen in Figures 1, 2 and 3, there appears to be a clear superiority of performance in the drugged conditions to that in control conditions. By binomial analysis the meprobamate result for each of the three animals is significant at better than the 0·01 level.

FIGURE 1

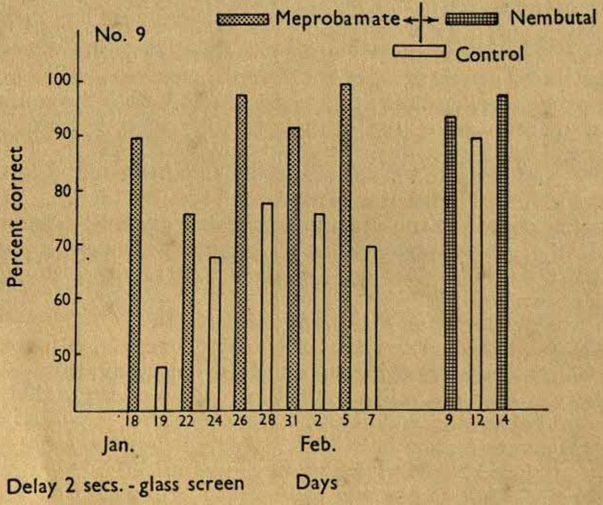


There was no difference between performance with isotonic saline and the drug vehicle, polyethylene glycol 400 (identified as PEG 400 in Figure 1).

Nembutal appears to be just as effective as meprobamate in increasing delayed response performance, for the dosages used.

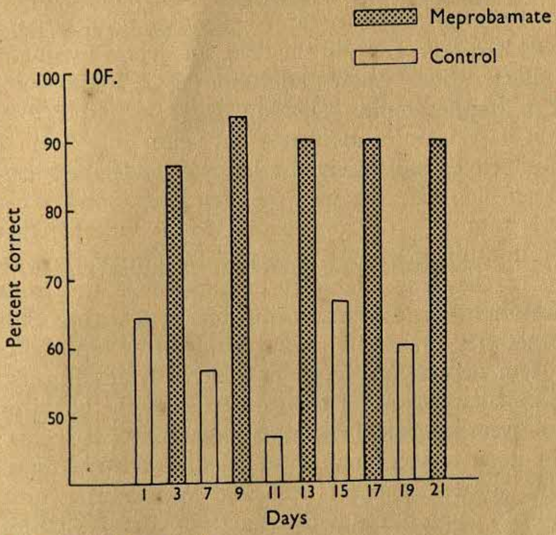
Monkeys Nos. 8 and 9 were given a single Ritalin session, not shown on the figures. In both cases there appears to have been a depression of performance below

FIGURE 2



the saline control levels just preceding and following it. With No. 8, the three values saline, Ritalin, saline) were 88 per cent, 76 per cent and 84 per cent. With No. 9 they were 90 per cent, 62 per cent and 90 per cent.

FIGURE 3



I sec. delayed response

DISCUSSION

The evidence seems to indicate that meprobamate, a drug which in the dose range used is ineffective in reliably improving delayed response performance in normal animals, significantly improves the performance of monkeys suffering from delayed response deficit by virtue of frontal cortex resection. It should be emphasized that the animals in the present study were performing, when undrugged, at levels of accuracy of the same order of magnitude (70 per cent) as the normal animals in the earlier study (Gross and Weiskrantz, 1961). The improvement in frontal animals in the present study, however, was seen when the test was made very simple—by having only a one sec. delay with two of the animals, and by using a transparent screen for the delay (of two sec. duration) with the third animal. Perhaps this is why Dean and Davis (1959) found an absence of meprobamate effect on frontal animals: they used a 15 sec. delay period, and in addition a lower dose. These authors, as well as Riopelle and Pfeiffer (1958), did, however, report beneficial effects of the "tranquillizer" reserpine, which we have found (Gross and Weiskrantz, 1961) to be similar to meprobamate in being ineffective in normal monkeys.

While there appears to be antagonism between frontal lesions and meprobamate (and reserpine), it is not possible on the present evidence to state whether this antagonism is or could be complete. While it is true that the drug effect was obtained only when the task was made very simple, it should be appreciated that normal monkeys generally require several hundred trials to learn, say, a five-second response task to a reliably high level of performance. Here, all of the beneficial effects of the drug had to be obtained within single sessions of 50 trials. Two of the animals had to overcome a long history of performing only at a chance level with a five-second delay.

The improvement seen with Nembutal suffers from the same logical flaw pointed out in the Introduction: it is not known whether the effect is not a general one, independent of frontal pathology. This would be simple enough to put to test, and we hope to do so soon. But it should be mentioned that earlier results with Nembutal, while widely quoted, were very far from consistent. In Wade's study (1947), for example, the improvement was only slight and, in the case of one of her two animals, it continued even after the drug was withdrawn, suggesting simply a gradual post-operative recovery. Pribram (1950) obtained positive results with Nembutal, but only after the animals had been kept on the drug for over a two-month period: there was no immediate effect. Blum, Chow and Blum (1951) found no effect on frontals either of Nembutal or amphetamine. Mishkin, Rosvold and Pribram (1953) found a slight improvement in 3 out of 6 monkeys, but again only after some delay. One of the animals who improved had received a lesion outside the focus for the delayed response deficit. Given such a history we were surprised to find so clear an indication of improvement by Nembutal. It is not possible to say whether this was due to our simpler testing conditions or an earlier history of meprobamate treatment.

What properties of the drug enable it to antagonize the frontal deficit? One possibility which has often been suggested in connection with Nembutal and frontal lesions is that hyperactivity caused by the latter might be reduced by the former—a central nervous system depressant. And in general tranquillizers, particularly at moderately high doses, share many of the properties of the barbiturates. However, even if meprobamate were known to reduce hyperactivity, it seems fairly clear that hyperactivity is not itself a necessary or sufficient condition for delayed response impairment following frontal lesions (Gross and Weiskrantz, 1964). The delayed response impairment can be produced without activity changes (Miles and Blomquist, 1960; Lawicka and Konorski, 1959; Glickstein, Arora and Sperry, 1960; Rosvold and Delgado, 1956; Weiskrantz, Mihailovic and Gross, 1962). Hyperactivity can occur without delayed response impairment (Ruch and Shenkin, 1943; Rosvold and Delgado, 1956; Dean and Davis, 1959). The activity of frontal monkeys can be drastically reduced pharmacologically without consequent improvement in delayed response (Pribram, 1950; Blum, Chow and Blum, 1951). It seems unlikely therefore, that activity changes brought about by the drugs can be responsible *directly* for the improvement in delayed response performance.

Indirectly, however, the suppression of hyperactivity might permit the frontal animal to develop a compensatory strategy involving bodily orientation. Lawicka and Konorski (1955) have shown that frontal dogs learn to compensate for the delay period. This possibility could be tested by studying the effects of the drug on performance with a *non-spatial* form of the delay test (e.g. object alternation; or stimulus baiting with random change of position of stimuli following delay).

A final interesting possibility stems directly from observations of other effects of the drugs themselves. If meprobamate is a genuine antagonistic of frontal cortex resection in the monkey, some insight may be gained of the frontal defect by considering the positive effects of the drug in the unoperated animal. In the study already cited (Gross and Weiskrantz, 1961) showing a lack of reliable effect of meprobamate and reserpine on delayed response performance by the unoperated animal, other evidence was submitted that similar dosages of the same drugs produce a severe deterioration in the performance on a discrimination task. Such evidence (which has since been extended, Weiskrantz and Baltzer, in press) was taken as support for the hypothesis that tranquillizers may interfere with the utilization of sensory input (Weiskrantz, 1957; Weiskrantz and Wilson, 1956). In turn, it may be suggested that the frontal monkey suffers from an excessive (and perhaps inappropriately ordered) intake of sensory input. Such a hypothesis is currently being examined experimentally, with several lines of evidence lending it strong support. For example, it has been shown that frontal monkeys take shorter "glimpses" of their environment in a Butler Box situation (Lindsley, Weiskrantz and Mingay, in preparation), that they manipulate objects in their environment in a more random order than control monkeys (Weiskrantz and Mingay, in preparation) and that their discrimination performance declines (relative to controls) as a function of the number of items in the set of discriminanda (Buffery, in preparation).

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DELAYED AUDITORY FEEDBACK AND SHADOWING

BY

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On the basis of speech disturbance under binaural DAF, groups of subjects of high and low susceptibility were formed. Both groups were required to shadow messages presented under four conditions: single message presented binaurally; message presented to one ear with either white noise, an irrelevant message, or delayed feedback of the repetitition of the message, presented to the contralateral ear. For both groups the number of errors (omitted words) increased significantly in the irrelevant message and the delayed feedback conditions as compared with the binaural or white noise conditions. There was no difference between the susceptible and non-susceptible groups in the binaural and white noise conditions, but the susceptible group showed a much larger increase in the irrelevant message and delayed feedback conditions. Implications of these findings for theories of DAF are discussed.

The experiment described in this paper resulted from a consideration of three empirically demonstrated facts. First, Cherry (1953) and Cherry and Taylor (1954) showed that it is possible to repeat accurately a message presented to one ear while an irrelevant different message is presented simultaneously to the other (dichotic stimulation). However, the speaker will be unable subsequently to report the content of the distracting message and may even be unable to specify its language or the sex of the speaker. In repeating the message, the technique known as shadowing is used, that is, the subject does not wait for a sentence or phrase to end before repeating the message, but attempts to keep as closely "on the heels" of the speaker as possible. Second, Cherry and Sayers (1956) have shown that stammering may be totally inhibited if the stammerer is required to shadow a message which he does not see, in other words, stammering may be abolished if a subject is prevented from listening to his own voice. This finding has been amply confirmed by Shane (1955) and by Maraist and Hutton (1957). Third, it is a well-established fact that normal speech may be seriously interfered with by means of the technique known as delayed auditory feedback (DAF) (Yates, 1963*a*). However, one of the most striking features of this phenomenon relates to the very marked individual differences in susceptibility to breakdown under DAF. While some subjects are so severely affected as to become almost incomprehensible, others are very little affected in their speech, although the experience is uniformly acknowledged to be stressful one.

No convincing explanation has yet been put forward as to why some subjects are highly susceptible to DAF, while others are not. It seems clear that personality factors play some role, but probably a minor one (Yates, 1963*b*). The most obvious possibility is that susceptibility is a function of the degree of dependence on auditory feedback for the normal monitoring of speech as compared with dependence on kinaesthetic and sensory feedback. In the former case, disruption of the normal auditory feedback relationships would be seriously disturbing, but in the latter case the effect would be less since the kinaesthetic feedback would remain normal (it would be expected that the speech of these subjects would be seriously disrupted if kinaesthetic feedback were interfered with). It should be noted that it is the *disruption* rather than the *absence* of feedback which produces the disturbance.

The experiment reported in this paper had two aims. The first was to discover whether subjects highly susceptible to speech disturbance under DAF would also find it more difficult to shadow verbal material under distracting conditions as compared with subjects not susceptible to DAF. The second aim was to discover whether highly susceptible subjects would be more resistant to the effects of DAF under a special shadowing condition. If these subjects are presented with a message in one ear which they must repeat using the technique of shadowing; and if their repetition of this message is presented to the other ear while they are still speaking but with a short delay (DAF); then the speech of these subjects should not be disrupted. This prediction follows from the finding by Cherry that repeating a message presented to one ear makes it very difficult to attend to what is happening in the other ear. Hence, provided the subjects are able to shadow successfully, they will be unaware of the delayed feedback being presented in the other ear and will be unaffected by it.

In order to achieve the second aim adequately, it is clearly necessary that at the time of the critical test (the presentation of a message in one ear, with delayed feedback of the repeated message in the other) the subjects should be able to repeat messages by shadowing successfully, since otherwise the performance in the critical condition might be solely a function of ability to shadow—or at least the effects of shadowing skill and susceptibility to DAF would be confounded. Hence, the training in shadowing was given first and was followed by the delay condition in all subjects.

METHOD

Selection of Extreme Groups

The extreme groups were chosen from 66 subjects, all University students who had no prior experience of DAF. In preliminary testing, all subjects first read a set of 36 simple sentences, chosen from Fairbank's manual (Fairbanks, 1960). Six groups of six sentences each were used, one sentence in each group illustrating the use of nasal m, n, η, and stop p, b, and t. The sentences were modified where necessary so that each contained nine syllables. After first reading all 36 sentences under no delay (but wearing headphones) the subject read the first 18 sentences again, one group under a delay of 150 millisec., a second group under a delay of 180 millisec., and a third group under a delay of 210 millisec. (The remaining sentences were for use in a separate experiment.) From the results of these repeated readings, two scores were derived—average increase in the number of errors under delay; and average time-ratio (time to read sentence under delay divided by time to read the same sentence under no delay). From a scatter diagram of error-increase plotted against time-ratio the 12 subjects showing most disturbance, and the 12 subjects showing least disturbance under DAF, were chosen, utilizing both criteria. The average error-increase and time-ratio scores for the chosen extreme groups are shown in Table I. It is clear that satisfactory discrimination was obtained.

TABLE I

Error and Time-Ratio Scores for Susceptible and Non-Susceptible Groups

Score		*Group*	
		Susceptible	*Non-susceptible*
Error Increase	Mean	8·83	0·17
	S.D.	3·96	0·36
	Range	6·00–18·00	−0·33–0·67
Time-Ratio	Mean	2·07	1·36
	S.D.	0·46	0·61
	Range	1·12–2·88	1·14–1·73

Method

Training in shadowing was carried out approximately one month after the preliminary procedure described above. The technique was explained verbally, and then illustrated by means of number sequences and one group of the short sentences previously used. This part of the procedure was informal and advice as to how to shadow most effectively was given. No subject had any difficulty in repeating either the numerals or the sentences under these informal conditions.

The procedure described below (see Table II for a summary) was then followed in the same order for all subjects.

TABLE II

EXPERIMENTAL CONDITIONS

Condition	*Procedure*	
	Ear 1	*Ear* 2
I	Shadowed Message	Shadowed Message
II	Shadowed Message	White Noise
III	Shadowed Message	Irrelevant Message
IV	Shadowed Message	DAF of Repetition of Shadowed Message

Condition I: Binaural shadowing. Four messages were presented successively binaurally to the subject through the headphones. Each message was preceded by a "ready" signal. Reproduction of the final two messages was recorded on tape. The level at the headphones was set at 85db as indicated by a Dawe Sound Level Meter, Type No. 1400E, which was acoustically coupled to one earphone of the headphone set. The coupling was by a sound column of one half-inch between the microphone and the headphone. These arrangements presented to the subject a message heard at a moderately loud but comfortable level.

Condition II: Uniaural shadowing with contralateral white noise. Four messages were presented successively to one ear, with white noise being presented simultaneously in the other ear. Two of the messages were presented to the left ear, with white noise in the other, and vice versa. The white noise was presented at a level which would not disturb the subject nor mask his perception of the message. The ready signal was presented in the same ear as the message and the subject was informed, before each message began, which ear would receive the message. Reproduction of the final two messages was recorded.

Condition III: Uniaural shadowing with contralateral irrelevant message. Four messages were presented successively to one ear, while a different message was simultaneously presented to the other ear. Again, the subject was informed which ear to attend to and the irrelevant message was presented at a level slightly lower than the message being repeated. The ready signal appeared in the same ear as the message to be shadowed, which also began about one second before the irrelevant message to aid identification. The irrelevant message ended about three seconds after the shadowed message. No subject had any difficulty in identifying the message to be repeated under these conditions. Reproduction of the final two messages was recorded.

Condition IV: Uniaural shadowing with contralateral delayed auditory feedback of repeated message. A message was presented to one ear (left or right, alternately) and the subject's repetition of the message was presented to the other ear with a delay of 180 millisec. An automatic volume control maintained the level of the delayed feedback equal to that of the message in the other ear. The subject was instructed to attend to the message in the appropriate ear but was not informed what would occur in the other ear. Two sentences were used. Reproduction of both messages was recorded.

The materials used in these four conditions consisted of sentences of 20 to 35 words in length, chosen partly (with modifications) from the "Rainbow Passage" (Fairbanks, 1960) and partly from novels. The material was relatively simple and did not present any difficulties in pronunciation per se.

The protocols were scored for accuracy of reproduction of the material shadowed. An error was recorded for each word omitted from the reproduction. For each subject a percentage omission score for each condition was calculated.

RESULTS AND DISCUSSION

Table III shows the mean percentage omission scores in the four conditions for the susceptible and non-susceptible groups. It is clear that an adequate test of the second aim could not be obtained, since neither group learned to shadow with satisfactory efficiency prior to the final condition, as indicated by the omission scores for conditions I, II, and III. Table III indicates that dichotic binaural shadowing, and shadowing a message in one ear while a masking tone is presented in the other, is relatively easy. However, the introduction of a competing message in the other ear presents a much more difficult task for both susceptible and non-susceptible groups.

TABLE III

PERCENTAGE OMISSION SCORES UNDER FOUR SHADOWING CONDITIONS

Condition	*Group*			
	Susceptible		*Non-susceptible*	
	Mean	S.D.	Mean	S.D.
I	1·41	1·92	0·74	0·54
II	2·66	1·41	2·77	2·78
III	8·57	6·56	4·57	3·64
IV	15·21	5·62	10·62	4·58

Analysis of variance of the data is presented in Table IV. The significant main effects indicate that the shadowing conditions are of unequal difficulty and that the susceptible group is significantly poorer at shadowing than the non-susceptible group.

TABLE IV

ANALYSIS OF VARIANCE FOR TWO GROUPS, FOUR CONDITIONS (REPEATED MEASURES)

Source of variation	*Sums of squares*	*df*	*Mean square*	*F-Ratio*	*p*
Between Subjects	679·10	23			
A (Susceptibility) ..	120·24	1	120·24	4·73	<·05
Subjects within groups	558·86	22	25·40		
Within Subjects	2903·21	72			
B (Conditions) ..	2009·11	3	669·70	56·04	<·001
AB	105·36	3	35·12	2·94	<·05
B × Subjects within groups	788·74	66	11·95		

The significant AB interaction indicates that the susceptible group finds shadowing significantly more difficult than the non-susceptible group when one ear is presented with a competing message, or with DAF of the repetition of a message, but not otherwise. Thus, there is little difference in efficiency of shadowing between the groups in the first two conditions, but a severe decline in efficiency occurs in the susceptible group when competing verbal material is introduced into the other ear; and this decline is greater than that experienced by the non-susceptible group.

This differential efficiency in shadowing with dichotic stimulation involving competing messages may be of importance in understanding extreme susceptibility to DAF. When subjected to DAF, a subject is required to deal with competing and contradictory sources of feedback from a single motor event—that is, he receives the auditory feedback in a time sequence that differs from the remaining (bone-conducted and sensory) feedback. It also differs, of course, from the *expected* pattern of auditory feedback, based on previous experience. His task then involves either restoring the time relationships artificially, or inhibiting the distorted feedback and relying on the non-distorted. Goldiamond, Atkinson and Bilger (1962) have suggested that the time-relationships may be artificially restored (i.e. the feedback and output speech units will again overlap) if a subject slows down his speech-rate by prolonging the speech-units excessively. It is probably the case that subjects who are able to resist DAF do so in part by utilizing this technique, whereas highly susceptible subjects are unable to accomplish this for reasons which are as yet largely unknown. However, as earlier argued, non-susceptible subjects may also be less dependent on the auditory channel and hence less affected by its distortion.

The results of this experiment, and those of earlier ones on dichotic stimulation, may be discussed in the context of Broadbent's (1958) filter theory. Utilizing the results of experiments in which different digits were presented simultaneously to each ear, he argued that two kinds of mechanisms must be involved in short-term reproduction of such material. The first mechanism, a "p-system," passes information successively only, that is, it is a single-channel system. The second mechanism, an "s-system," temporarily stores information until a "vacancy" occurs in the single channel p-system which will then process it. Both systems may be involved in ordinary sequential performance where a single stream of information is being processed as well as in situations involving more than one source of information.

It is important to notice, however, that Broadbent's filter theory was developed to deal with the observed recall order of pairs of digits presented dichotically, i.e. where the subject has to attend to both channels. The results of the present experiment represent rather a situation in which the selector mechanism fails to operate successfully. Broadbent himself, on the basis of many experiments, has suggested that an important variable in this respect may be the similarity of content of the two messages. The relationship may well be curvilinear in nature, with maximal interference occurring when the competing message is moderately dissimilar to the principal one and minimal interference when the competing message is identical with, or completely different from, the principal one. The effects of DAF, however, suggest that the time relationships may also be a crucial factor in breaking down effective selection, since in this case, the content in the feedback channels is the same, only the time relationships varying.

SUPPLEMENTARY EXPERIMENT

In the hope of obtaining a test of the second aim (that the effects of DAF would be absent in the fourth condition if shadowing skill had been previously mastered) a supplementary experiment was carried out. Eleven of the original twelve highly susceptible subjects were available. The following training procedure was used. In Condition I (binaural single message presentation) the first three sentences were presented repeatedly until subject reproduced all three in the same trial without error. The fourth sentence was then presented twice. It is obvious that repeated presentation of the sentences could lead to successful reproduction through rote memory rather than shadowing. However, if the fourth sentence was also repeated successfully, this would to a considerable degree rule out the memory factor as the most probable reason for success. The same procedure was then followed for Conditions II and III. In Condition IV, an additional sentence was added to the original two for training purposes, and a test trial of two repetitions was then given on a fourth sentence which the subject had not heard before.

The results of this experiment are shown in Table V. The criterion used during the training period was the number of trials required to reach one perfect repetition while in the test period the score was the number of omissions averaged over the two presentations. All subjects except one attained the training criterion of perfect performance in Conditions I, II, and III. In Condition IV, however, five subjects were unable to achieve the training criterion in from four to eight trials (training was stopped at an earlier point for some subjects who seemed to be finding the task too stressful). However, they all were given the test trials even if they did not reach the training criterion. In Table V the two groups of subjects are shown separately.

TABLE V

Training and Testing Results in Supplementary Experiment

Subject	*No. of trials to one perfect repetition*				*Percentage omissions over two trials*			
	Training Conditions				*Test Conditions*			
(Group 1)	I	II	III	IV	I	II	III	IV
4	1	4	5	3	0·00	23·08	0·00	5·17
5	3	5	3	4	0·00	0·00	3·33	5·17
7	3	3	6	4	0·00	28·85	4·55	15·52
8	2	2	3	8	1·92	1·92	15·15	0·00
9	1	2	2	3	0·00	1·92	1·52	3·45
11	1	3	3	3	0·00	0·00	18·18	0·00
(Group 2)								
1	3	5	5	(8)	3·85	0·00	0·00	12·07
2	5	2	3	(7)	0·00	11·54	6·06	5·17
3	1	2	(7)	(5)	0·00	0·00	6·06	5·17
6	2	3	4	(5)	0·00	0·00	10·61	0·00
12	1	3	3	(4)	3·85	0·00	4·55	15·52
Mean (Gp 1)	1·83	3·17	3·67	4·17	0·32	9·29	7·12	4·88
Mean (Gp 2)	2·40	3·00	4·40	—	1·54	2·31	5·45	7·59

It is clear that, even with the additional practice, stable shadowing performance was not completely established. In the crucial condition, however, there is no doubt that a substantial reduction in omissions has taken place, even in the sub-group which did not succeed in meeting the training criteria. The omission of roughly one word in every twenty does represent a substantial level of achievement in shadowing when the repeated message is played back into the contralateral ear with a delay.

A completely satisfactory test of the second hypothesis, however, does demand that omissions be reduced to zero. When this level is attained, the records can then be scored in the usual way for measurement of the effects of DAF. Otherwise it is impossible to separate out the effects of DAF from the effects of a competing message.

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SHORT-TERM TEMPORAL CHANGES IN FREE RECALL

BY

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An experimental study of short-term memory for lists of familiar English words is reported. Lists of 10, 20, and 30 unrelated words were presented at a 1-sec. rate. Retention was measured by free recall after intervals of 0, 15 and 30 sec. A counting task was used to prevent rehearsal during the retention interval. The absolute level of recall increased with length of list whereas the percentages retained showed the reverse trend. The recall scores decreased steadily as a function of retention interval, with the rates of forgetting comparable for the three lengths of list. The decline in the amount recalled was due in large measure to the loss of the terminal items in the list. Consequently, the pronounced recency effect present on the immediate test of recall was progressively reduced as a function of time. By contrast retention of the initial part of the list was relatively stable. These variations in rate of forgetting are attributed to differences among serial positions in susceptibility to proactive inhibition.

INTRODUCTION

This experiment investigates short-term forgetting in the free recall of lists of unrelated words. After a single presentation an immediate test typically shows recall to be highest for the initial and terminal parts of the list, with the effect of recency considerably more pronounced than that of primacy. The predominance of recency over primacy increases with the length of the list (Murdock, 1962). A greater recency than primacy effect is to be expected on the assumption that the serial position curve reflects variations in the amount of interference at recall. Each member of the series is subject to retroactive inhibition (RI) from subsequent items and proactive inhibition (PI) from preceding items. As the end of the series is approached, the number of items interpolated between presentation and recall decreases; at the same time the number of prior items increases. Hence RI must be expected to decline and PI to grow as a function of serial position. The relative effectiveness of the two types of interference depends, however, on the length of the retention interval. Whereas RI is maximally effective immediately after an interpolated activity, PI develops gradually after the end of learning. The fact that PI is virtually absent at first but increases steadily as a function of time has been demonstrated for both short-term memory (Keppel and Underwood, 1962) and long-term memory (Postman, 1962). Thus, the terminal items of the list should have a clear advantage in an immediate test of recall since they are subject to little or no RI, and PI is minimal at the beginning of the retention interval.

The earlier an item appears in the series the more likely it is that the interval between presentation and test is sufficiently long for PI to become effective at the time of recall. The primacy effect may, therefore, reflect relative freedom from interference by preceding items. A reduction in PI is, however, necessarily accompanied by an increase in RI. The fact that the primacy effect is smaller and declines more precipitously than the recency effect is in accord with the assumption that an immediate test of recall is less influenced by variations in the amount of PI than of RI.

A recency effect is also to be expected on the assumption that the trace of each item begins to decay immediately after presentation (Brown, 1958). The later an item is presented the shorter is the period of decay, so that the probability of recall should increase steadily as a function of serial position. A primacy effect is not predicted unless it is assumed that the initial items are rehearsed during the exposure of the remainder of the series (cf. Raffel, 1936; Welch and Burnett, 1924).

Variations in the length of the retention interval will be determined not only by the serial position during presentation but also by the order in which items are recalled. Thus, terminal items would lose much of their advantage if the series were recalled in the order of presentation. In general there is, however, a high correlation between probability of recall and order of recall (Bousfield, Cohen, and Silva, 1956; Deese, 1957; Deese and Kaufman, 1957). Thus, the terminal items of a series of unrelated words have the highest probability of recall and also are usually recalled first. Under these circumstances the availability of the items at the beginning of the test period and the order of recall have parallel effects on the shape of the serial position curve.

The present analysis suggests that the magnitude of the recency effect should decline progressively as the interval between the end of presentation and recall is lengthened. Since PI increases as a function of time, the terminal part of the list, which is subject to maximal interference from prior items, should gradually lose its advantage. A reduction in the recency effect must, of course, also be predicted on the assumption of trace decay. The primacy effect should be more stable since PI is minimal for the initial items and RI does not increase during the retention interval. Rehearsal of the initial items during the presentation of the remainder of the series would add to the stability of the primacy effect. These implications are tested in the present study. The variables manipulated in this experiment included length of list as well as the duration of the interval between presentation and test. Since the amount of PI falling on the terminal items increases with length of list, the rate at which the recency effect is reduced should vary directly with length.

METHOD

Materials

Three different lengths of list were used, viz. 10, 20 and 30 items. All items were common English words drawn from the AA and A range of the General Count of Thorndike and Lorge (1944). The same words were used equally often for each length of list: to each 30-word series there corresponded three different 10-word and 20-word series in which the same items were used in appropriate combinations.

Design

A different group of subjects was used for each length of list. All subjects were given six tests of free recall at each of three retention intervals, viz. 0, 15, and 30 sec. after the end of the presentation of the list. The sequence of 18 tests was counterbalanced so that (*a*) the three intervals were used equally often in each ordinal position, and (*b*) a given interval was preceded equally often by each of the three intervals. In order to satisfy the latter requirement an initial buffer list was added for which the interval was the same as for the first test list. Within each group the order of the word lists was kept constant.

Procedure

The lists were presented at a 1-sec. rate on a memory drum. For the delayed tests, the retention interval was filled with counting backwards in steps of three (cf. Peterson and Peterson, 1959). Immediately after the presentation of the last item the experimenter spoke a three-place number indicating the starting point of the series to be used in counting backwards; the subject then continued responding aloud until a bell signalled the beginning of the recall test. The same signal was used for the immediate test. The subject was allowed 1 min. in which to call out as many words as he could, in any order in which they occurred to him. The time allowed for the test was sufficient under all conditions since recall had been found to be negligible at the end of a minute. Two bells signalled the end of the recall period. The subject's responses were written down by the experimenter and also tape-recorded for later verification. The interval between the end of the recall test and the presentation of the next list was 5 sec.

Subjects

There were three groups of 18 subjects, one for each length of list. The subjects were undergraduate students who were assigned to conditions in blocks of three, with one subject per block from each group. The order of assignment within blocks was random.

RESULTS

Amount recalled

Table I shows the mean number of items recalled as a function of retention interval for each length of list. The absolute level of the scores rises significantly with length of list ($F = 14{\cdot}56$, $d.f. = 2/51$, $p < 0{\cdot}001$), in agreement with the finding that the amount recalled increases with total presentation time (Murdock, 1961). However, the percentages recalled, which are also listed in Table I, show the reverse trend. For each length of list there is a substantial amount of forgetting on the delayed tests, with the retention losses showing some negative acceleration. The differences among retention intervals are highly significant ($F = 67{\cdot}88$, $d.f. = 2/102$, $p < 0{\cdot}001$). The trends in the absolute amount lost are closely comparable for the three lengths of list and do not differ significantly ($F < 1$). Consequently, the drops in the percentages recalled are inversely related to length.

TABLE I

MEAN NUMBER OF ITEMS RECALLED AS A FUNCTION OF LENGTH OF LIST AND RETENTION INTERVAL

Interval (*sec.*)	*Length of list*					
	10		20		30	
	Mean	*Per Cent*	*Mean*	*Per Cent*	*Mean*	*Per Cent*
0	5·58	55·8	6·20	31·0	7·79	25·9
15	4·06	40·6	5·05	25·2	6·25	20·4
30	3·55	35·5	4·26	21·3	5·89	19·5

Serial position curves

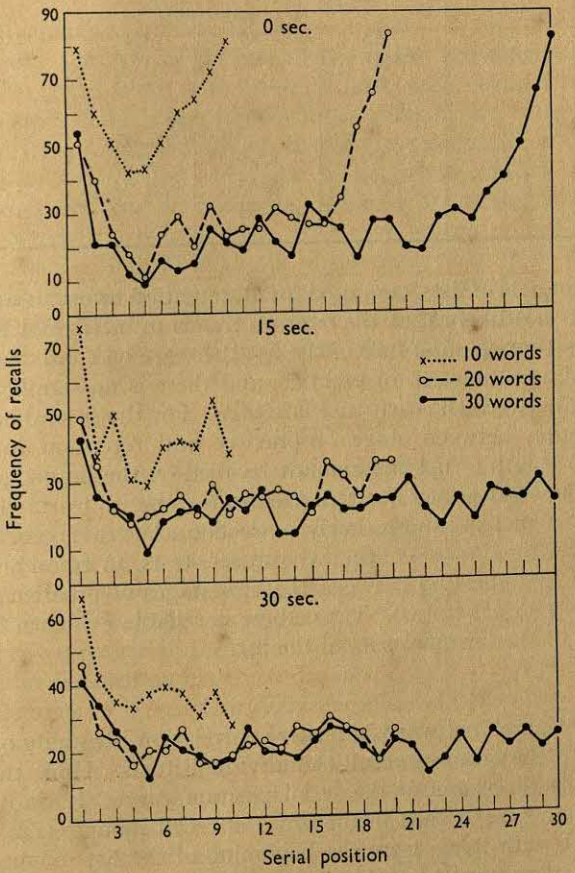
Figure 1 shows the frequency of recalls as a function of serial position during presentation. The total frequencies summed over the six tests are plotted. The curves obtained immediately after the end of presentation (0 sec.) show a pronounced recency effect in each case. There is also a primacy effect, which is relatively greater for the 10-item list than for the two longer series. The recency effect declines steadily as a function of the retention interval and is entirely lost on the 30-sec. test. By contrast, the losses in the initial positions of the list are relatively minor.

For purposes of evaluating the differences in rate of forgetting as a function of serial position, separate scores were determined for the first and second half of each list. Table II presents these scores for the early and late stages of practice (Tests 1–3 and 4–6). The over-all means are also shown and will be considered first. The decline in retention scores as a function of time is consistently greater for the second than the first half of the list. A test of trend shows this difference in rate of forgetting between the two parts of the list to be highly significant ($F = 47{\cdot}74$, $d.f. = 1/51$, $p < 0{\cdot}001$). The relative advantage of the first half increases with length of list. Specifically, as the list becomes longer, the losses over time become less for the first half of the list and greater for the second half. In the trend analysis the interaction of half of list with length closely approaches statistical significance ($F = 3{\cdot}08$, whereas 3·18 is required at the five per cent. level for 2/51 *d.f.*).

Cumulative interserial PI

Since each subject was tested repeatedly, there was opportunity for the development of interserial PI. To test for the presence of cumulative interference from prior lists it is necessary to compare the retention scores at successive stages of practice. It is known that PI increases as a function of (*a*) the number of prior lists and (*b*) the length of the retention interval. Hence, the difference between the immediate and delayed tests should increase with the number of prior lists (cf. Keppel and Underwood, 1962). In such an analysis practice effects are held constant when the ordinal positions of the different retention intervals are fully counterbalanced.

FIGURE 1



Serial position curves in recall as a function of length of list and retention interval.

Since forgetting was greater for the second than the first half of the list, PI effects were considered for the two parts separately. The relevant scores permitting a comparison between the early and late stages of practice are shown in Table II. It should be noted that whereas the sequence of retention intervals was counterbalanced, the order of lists was kept constant. In view of possible differences in list difficulty the absolute amounts of PI cannot, therefore, be assessed with precision.

TABLE II

MEAN NUMBER OF ITEMS RECALLED FROM FIRST AND SECOND HALF OF LIST IN EARLY AND LATE STAGES OF PRACTICE

ENTRIES ARE MEAN SCORES PER LIST

Tests	*First half of list*			*Second half of list*		
	0 *sec.*	15 *sec.*	30 *sec.*	0 *sec.*	15 *sec.*	30 *sec.*
			10 *words*			
1–3	2·61	2·06	1·94	2·80	1·91	1·56
4–6	2·48	2·09	2·00	3·28	2·06	1·59
Over-all ..	2·54	2·08	1·97	3·04	1·98	1·58
			20 *words*			
1–3	2·76	2·43	2·20	3·41	2·52	2·30
4–6	2·28	2·41	2·00	3·96	2·74	2·02
Over-all ..	2·52	2·42	2·10	3·68	2·63	2·16
			30 *words*			
1–3	2·87	3·00	2·96	4·74	3·33	3·02
4–6	3·13	2·98	3·13	4·83	3·20	2·67
Over-all ..	3·00	2·99	3·04	4·78	3·26	2·84

However, the same sets of lists were used for all retention intervals at a given stage of practice, and it is not likely that the over-all trends in interserial PI are subject to systematic biases. For the first half of the list there are no consistent changes in the amounts retained as a function of practice, and there is no significant difference in rate of forgetting between the early and late tests. For the second half of the list the expected interaction between stage of practice and retention interval is clearly present. At each length of list the amount recalled on the immediate test increases with practice. These gains may be attributed to learning to learn. The scores on the delayed tests either increase more slowly or decline. The divergence from the 0-sec. condition is greater for the 30-sec. than the 15-sec. tests. A trend analysis shows the difference in rate of forgetting between the two stages of practice to be significant ($F = 6{\cdot}88$, $d.f. = 1/51$, $p < 0{\cdot}02$). Thus, there is reliable evidence for interserial PI only in the recall of the terminal part of the list.

Order of recall

In the recall of unrelated words a postive correlation is usually obtained between the frequency and the order of recall of individual items. Thus, the order of recall helps to determine the shape of the serial position curve. If the usual correlation holds in the present case, a shift in the order of recall should occur as a function of retention interval, with items from the beginning of the list moving forward in the order of recall as the interval increases. When the absolute levels of retention vary widely, comparable indices of the order of recall are not readily available. To obtain a rough approximation to such an index, each protocol was classified according to whether the very first item recalled came from the first or second half of the list. Each subject was then assigned a score based on the number of times he gave an item from the second half of the list as his first response. Since there were six tests, the value of this recency index could vary from zero to six. The mean scores are shown in Table III. As expected, the value of the recency index declines significantly

TABLE III

RECENCY INDEX IN ORDER OF RECALL

ENTRIES ARE MEAN NUMBERS OF TESTS IN WHICH AN ITEM FROM THE SECOND HALF OF THE LIST WAS THE FIRST ONE RECALLED

Length of list	*Interval (sec.)*		
	0	15	30
10	3·72	3·25	3·19
20	5·00	3·67	2·47
30	5·61	3·42	2·86

as a function of retention interval ($F = 33{\cdot}38$, $d.f. = 2/102$, $p < 0{\cdot}001$). A trend analysis shows that the rate of decline of the index increases with length of list ($F = 11{\cdot}52$, $d.f. = 2/51$, $p < 0{\cdot}001$), in agreement with the difference observed in the amounts recalled. Thus, the order of recall reflects the changes in the relative availability of initial and terminal items. It must be recognized, of course, that items from the second half of the list constitute progressively smaller percentages of all the items recalled as length of list increases. Some decline in the recency index would, therefore, be expected on that basis alone. However, whereas the drop in the per cent. of items from the second half of the list is of the order of 10 to 15 during the 30-sec. interval, the decline in the recency index is of a comparable magnitude only for the 10-word list; for the 20-word and 30-word lists the index falls by approximately 50 per cent. Thus, for the longer lists at least, the shift in the order of recall would appear to be systematic.

DISCUSSION

When the opportunities for rehearsal are minimized, there is substantial forgetting for lists of common English words during the period immediately after the end of presentation. These retention losses are due in large measure to a drastic reduction in the probability of recall of the terminal items. Thus, the characteristic recency effect in the free recall of unrelated words is shown to be quite transitory. By contrast, the primacy effect, while initially less pronounced, is considerably more stable. These shifts in the relative strength of the initial and terminal parts of the list are reflected in both the amount recalled and in the order of reproduction of the items. A similar difference in resistance to interference between initial and terminal items was shown in the recall of paired associates by Tulving and Arbuckle (1962).

It was shown earlier that this pattern of retention losses is predictable on the basis of general principles of interference, and specifically of the temporal characteristics of RI and PI. It was assumed that progressive increases in PI falling on the terminal items would lead to a reduction in the recency effect as a function of time. The fact that the rate at which the recency effect is reduced increases with length of list is consistent with this assumption since the amount of interference from prior items should vary directly with length. It is possible that RI from the counting task contributes to the observed retention losses. It is known, however, that in the absence of PI the amount of interference produced by this interpolated activity is negligible (Keppel and Underwood, 1962).

Certain additional features of the results require comment. First, not only does the rate of forgetting for the terminal items increase with length of list, but that for the initial items decreases at the same time. It is likely that item selection is responsible for the apparent beneficial effects of length on the retention of the initial part of the series. The amount of RI falling on the early items increases with length of list, i.e. the longer the list the greater is the amount of interference which initial items must overcome to be recalled on the immediate test. Thus, increases in length may lead to the progressive selection of items for strength and hence to resistance to further forgetting. Second, interserial PI, which is again in accord with general principles of interference, was largely limited to the recall of the terminal part of the list. An interpretation in terms of item selection may apply here as well: initial items which are resistant to RI from subsequent parts of the list are less subject to interserial PI than terminal items among which there is no comparable selection.

An attempt has been made to interpret the present results entirely within the framework of an interference theory of forgetting. It is recognized that the major finding of the study, viz., the rapid reduction of the recency effect over time, is also consistent with an hypothesis of trace decay, although some additional assumptions would be required to take account of the relative stability of the primacy effect. The failure of the over-all amounts of forgetting to increase with length of list and the presence of interserial PI also pose difficulties of interpretation from the point of view of trace theory. While the results of the present study cannot be regarded as theoretically critical, they add to the mounting body of evidence (cf. Postman, 1964) for the continuity of the principles of interference governing short-term and long-term retention.

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ITERATIVE TECHNIQUES FOR UNBIASED RATING SCALES

BY

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Three iterative techniques for neutralizing the effects of stimulus bias in category rating experiments were examined with a wide variety of stimulus variables. Under all conditions examined, the iterative techniques quickly led to a stable category estimation. This result was obtained for stimulus variables with strong measurement properties, e.g. length and weight; for stimulus variables with only ordinal properties, e.g. emery papers; and for stimulus variables with only nominal properties, where an ordered set is obtained only in the course of the category scaling, e.g. female profiles.

The psychological experimenter may unwittingly introduce various sources of bias into his experiment (Rosenthal, 1963). One such source, which is referred to here as *stimulus bias*, arises from the inevitably arbitrarily selected stimulus values at the beginning of the experiment. As an illustration of stimulus bias, Garner (1954) demonstrated that the reduction in sound level required for half loudness by a listener may be completely determined by the range of sound levels selected by the experimenter in the method of constants.

The aim of this paper is to explore techniques which neutralize stimulus bias. Since stimulus bias has been shown to be substantially more effective with category ratings than with magnitude estimation (J. C. Stevens, 1958), we shall restrict our examination to category rating scales.

The techniques considered here are *iterative* procedures in which the results of the first experiment modify the conditions of the second experiment, the results of the second experiment modify the conditions for the third, etc. S. S. Stevens (1955) was perhaps the first to advocate the systematic use of iterative experimentation and has been its most persistent and dedicated supporter (e.g. Stevens and Galanter, 1957; Stevens and Poulton, 1956; Stevens, Rogers, and Herrnstein, 1955).

The application of iterative procedures to the neutralization of stimulus bias stems from the subjects' strong expectations that each of the rating categories will be employed approximately equally often (Arons and Irwin, 1932; Stevens and Galanter, 1957). When this condition is not met, the subject may alter his category ratings in the direction of his expectations. Our basic starting point is that the effects of stimulus bias will be neutralized by iterative techniques which modify experimental conditions in the direction of the subject's expectations, i.e. in the direction of equal utilization of the several rating categories.

APPROACH

Our basic strategy of experimentation was deliberately to introduce stimulus bias by employing two sets of stimuli that were differentially weighted in opposite directions; and then to attempt to remove the bias by application of iterative techniques (described below). When successful, the entire set of operations will be referred to here as the *convergence* upon an unbiased rating scale.

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Method

With the exception of the rating of facial beauty (Figure 5), all tests of Figures 1–8 employed the method of category estimation (Stevens, 1958, Table 2) with a 7-point rating scale. Typically, the subject was first exposed to the extremes of the stimulus variable and was instructed that the lower extreme represented the lower end of rating category 1 and that the upper extreme represented the upper end of rating category 7, and that intermediate stimuli were to be assigned intermediate rankings. He was also told that more than 7 stimuli would be presented so that some rankings would necessarily be employed more often than others, that some rankings might not be employed at all, and that the extreme stimuli would be employed within the rating series.

Subjects

With the exception of the rating of facial beauty, subjects were 10–17 Royal Naval Ratings or postmen of similar educational standard, serving as subjects at the Applied Psychology Research Unit. Each of 11 stimuli was presented for rating two or three times. All subjects were tested individually. The order of presentation was scrambled among subjects.

Stimulus Ensembles

The stimulus ensembles employed in the tests of Figures 1–4 are schematically represented by the insert graphs. The abscissa of the insert represents the stimulus variable; the ordinate of the insert is a rank ordering of the stimulus variable. For example, the insert of Figure 4 represents two sets of 11 different proportions each: one set (in open circles) increases in steps of 0·05 between 0·0 and 0·45 to furnish 10 steps, plus the upper limit of 1·0; and one set (in filled circles) decreases in steps of 0·05 between 1·0 and 0·55 to furnish 10 steps, plus the lower limit of 0·0. The separate ensembles were:

a. Length of rods (Figure 1). Two sets of lengths of wooden rods were employed. The maximum length of each set was 11·25 in. The minimum length of set A was 6·75 in.; the minimum length of set B was 1 in. The initial sets of lengths are represented in the inserts of Figure 1. With the wider range, it was assumed that stimuli were drawn from a population of 21 rods, spaced approximately linearly or spaced approximately logarithmically.

b. Weight of canisters (Figures 2 and 6). Sand in paper canisters provided a set of weights ranging from 100 gm. to 174 gm, spaced in steps of about 2·3 per cent.

c. Marker position (Figures 3, 6, and 8). Cards were marked with a short, thin pencilled marker at right angles to a thin pencilled line of 100 mm long.

d. Relative proportion (Figures 4 and 6). Red and blue dots were drawn in ¼ in. cells of a 10 × 10 rectangular matrix.

e. Facial beauty (Figure 5). Twenty-four facial profile photographs of young women were employed as stimulus materials. The following procedure was first carried out in order to obtain different weighted sets of photos. The twenty-four photos were first presented to forty Cambridge University students, one-at-a-time, for absolute ratings upon a 10-point scale of facial beauty. The extreme cases were not defined. The resulting ratings (adjusted to an arbitrary rating scale) against the resulting rank-ordering of the photos are presented in Figure 5E. From the original rank-ordering, seven sets of photos were employed: three different sets of sixteen photos of Figure 5D and four different sets of photos of Figure 5H. Each set was presented to groups of 14–40 Cambridge University students for category sorting among six, or fewer, categories.

f. Grey papers (Figures 6 and 8). Twenty-nine grey papers ranged from white to black.

g. Emery papers (Figure 7). Sets of five or six emery papers, specified in terms of grit number, were employed. In judging the apparent roughness of emery papers, subjects were instructed to rub their fingertips lightly over emery papers presented one-at-a-time, and to estimate the roughness on a 7-point scale. The papers were not visible to the subject.

h. Sound level (Figure 8). The sound level of a 1000-cycle tone spanned the range of 10 db or 40 db.

i. Sound frequency (Figure 8). The frequency of a tone spanned a frequency range from 600–700 cps or from 300–3000 cps.

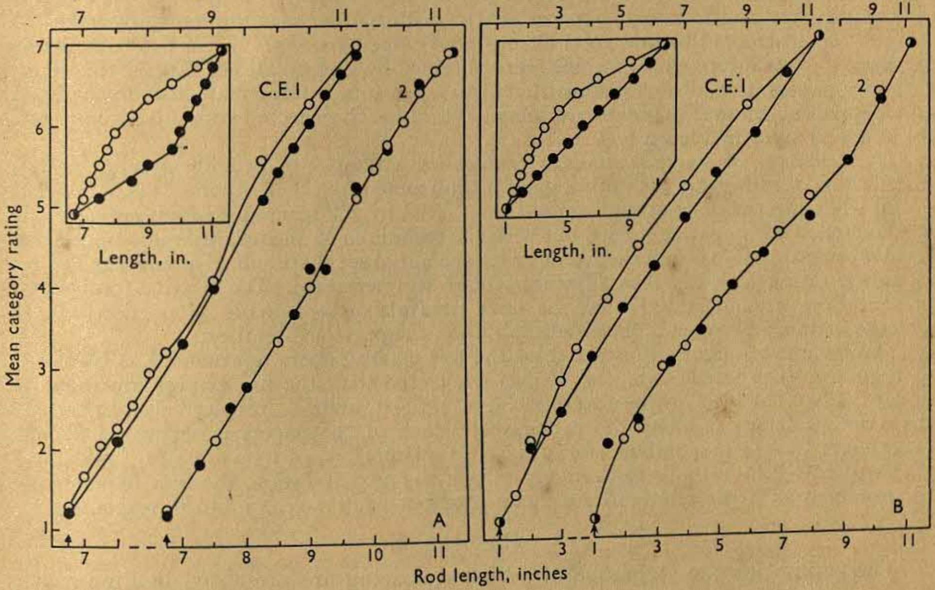
Iterative techniques

The *technique of adjusted spacing* attempts to provide a more uniform distribution of ratings by adding stimuli where the slope of the category scale is steep; and by deleting stimuli where the slope of the category scale is shallow. The technique is especially useful where intermediate positions in the stimulus domain are available and well-defined, as in many univariate stimuli such as length, weight, and duration.

The following steps are carried out in the technique of adjusted spacing. A category estimation is first carried out with each initial set of stimuli. The distance along the rating scale, associated with the upper and lower stimulus limits, is subdivided into (s-1) equal intervals, where s is the number of required stimuli. From the equally spaced ratings on the ordinate, horizontal projections are drawn to the category estimation, and vertical projections are dropped to the stimulus scale on the abscissa. Stimuli for the next iteration are interpolated along the stimulus scale. The technique is applied over and over again until a stable rating function is obtained.

The technique may be illustrated by a specific example. Consider the eleven lengths represented by the filled circles of the insert of Figure 1A. The mean category estimations to this set of lengths are plotted as the filled circles under C.E.1 in Figure 1A. The extreme ratings associated with this stimulus ensemble were 1·2 and 6·8. The distance along the rating scale, 5·6 (6·8–1·2), was subdivided into ten intervals, since we wished to obtain eleven new stimuli for the next iteration. The ordinate was then marked off in steps of 0·56 (5·6/10) from 1·2 to 6·8. Horizontal projections, parallel to the abscissa, were then drawn to the smooth curve drawn by eye through the points. Vertical projections parallel to the ordinate were dropped to the abscissa. The latter projections then defined the set of lengths available to the next iteration. For example, the stimulus corresponding to a mean category rating of 1·2 remained at 6¾ in.; that corresponding to a mean rating of 1·76 (1·2+0·56) was about 7¼ in. . . . , that corresponding to a mean rating of 6·8 remained at 11¼ in. This new set of eleven stimuli was then presented to subjects for a second category estimation, the results of which are presented as the filled

FIGURE 1



Application of the technique of adjusted spacing to the category estimation of length. Two sets of rods, A and B, differed in the range of lengths. The insert figures of Figures 1–4 represent the initial stimulus ensembles: the open dots represent one ensemble; the filled dots represent the other. The category estimations for the initial stimulus ensembles are represented under C.E.1, where the open and filled dots are associated with the correspondingly-coded ensembles represented in the insert. Further category estimations are represented by C.E.2, C.E.3, etc. (see text for details). All curves in Figures 1–8 have been fitted to the data by eye; they are without theoretical significance.

circles in C.E.2 in Figure 1A. In a similar manner, the eleven lengths represented by the open circles of the insert of Figure 1A were presented to another group of subjects for category estimation. The results are presented as the open circles under C.E.1 in Figure 1A. A new set of eleven lengths, with the extremes of the range held fixed, was obtained by the technique of adjusted spacing (described above). This new set of eleven stimuli was then presented to subjects for a second category estimation, the results of which are presented as the open circles in C.E.2 in Figure 1A. Further iterations were not considered necessary because nearly identical category estimations were obtained despite the large differences in the initial stimulus ensembles.

The *technique of category production* attempts to provide a "reasonable" distribution of stimuli by preliminary testing, and, thereby, avoid weighted stimulus distributions (Stevens and Galanter, 1957, 392–3).

The entire set of stimulus objects available to the experimenter was presented to the subject for inspection: lifted weights (25 weights ranging from 100 to 173 gm), grey papers (29 papers ranging from white to black), marker position (35 cards ranging from 0 mm to 100 mm marker position upon a 100 mm line), and relative proportion of blue dots (35 matrices ranging from 0 to 100 per cent). With marker position and proportion, the subject was instructed to find good examples of categories 2, 3, . . . , 6 between the end categories. Nine intermediate stimuli were obtained by interpolation. With lifted weights and grey papers, the subject was instructed to select nine intermediate stimuli to achieve equal apparent differences between successive stimuli. The selected stimuli are represented by plus (+) signs in Figure 6.

The *technique of unequal stimulus frequencies* attempts to provide a more uniform distribution of ratings by presenting some stimuli more often than others. It is particularly useful when intermediate stimuli cannot be derived by the technique of adjusted spacing.

The following steps are carried out with the technique of unequal stimulus frequencies. A category estimation is first carried out with the initial set of stimuli. The average distance along the rating scale between adjacent stimuli is determined. The relative frequency of presentation of each stimulus on the next iteration is then made proportional to the average distance between adjacent stimuli upon the previous iteration.

The technique may be illustrated by the five emery papers represented as black dots in Figure 7. The initial category ratings associated with the five papers are presented under C.E.1. The mean ratings were 1·0, 2·4, 3·5, 5·3, and 6·1, respectively. In terms of the average distances along the rating scale, each of the papers is separated from its neighbours by 1·4 (2·4 minus 1·0); 1·25 ((3·5 − 1·0)/2); 1·45 ((5·3 − 2·4)/2); 1·3 ((6·1 − 3·5)/2); and 0·8 (6·1 − 5·3), respectively. Upon the next iteration, the four papers would be presented with the relative frequencies of about 14, 12, 14, 13 and 8, respectively.

RESULTS

The results for the technique of adjusted spacing are presented in Figures 1–5, for the technique of category production in Figure 6, and for the technique of unequal stimulus frequencies in Figures 7 and 8. The results for the first category estimation, based upon the initial stimulus ensembles, are presented under C.E.1. The results for subsequent category estimations, based upon the stimuli derived by application of the iterative techniques, are labelled C.E.2, C.E.3, etc. The initial category estimation following category production is labelled C.E.1. The filled and unfilled circles refer to different stimulus ensembles, as illustrated in the insert graphs.

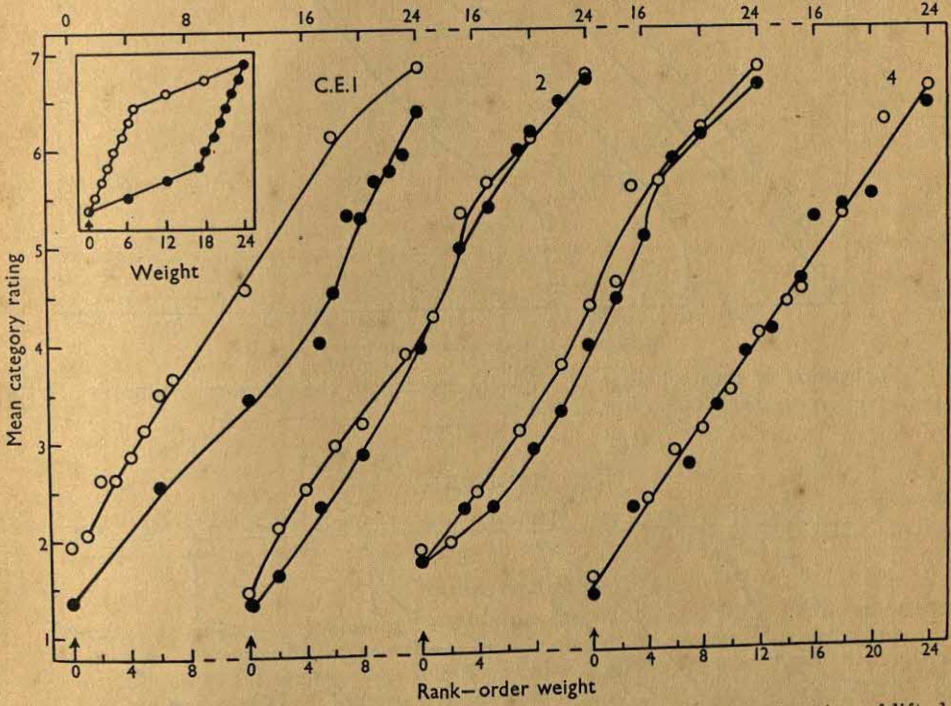
The results may be summarized succinctly. Convergence upon a stable category estimation is achieved by all three techniques for each of the stimulus ensembles examined.

The following remarks amplify the basic finding:

In the scaling of a restricted range of length (Figure 1) and weight (Figure 2), there is contraction of the range of available ratings, due to the failure to identify the extreme stimuli correctly. Convergence upon an unbiased category estimation is, nevertheless, achieved.

The results with marker position (Figure 3) and relative proportion (Figure 4) demonstrate convergence upon an unbiased category estimation for variables that are usually not considered as intensive variables (cf. related distinction between prothetic and metathetic variables, Stevens and Galanter, 1957, p. 377). Apparent convergence was obtained, despite the experimenter's initial failure to recognize the subjects' extreme sensitivity near the ends in the marker position or proportion series. Many subjects do not assign the extreme category ratings to any markers or proportions that are discriminably different from the end marker positions or from 0 to 100 per cent proportions. For such subjects the slope of the rating scale in the vicinity of the extreme stimuli is even steeper than shown in Figures 3 and 4. Apparent convergence may be obtained without fully developing the scale near the extreme marker positions or extreme proportions. The fourth category estimation of position (the X's of C.E.4 in Figure 3) was carried out after convergence was apparently achieved under the third category estimation. The fourth category estimation was

FIGURE 2



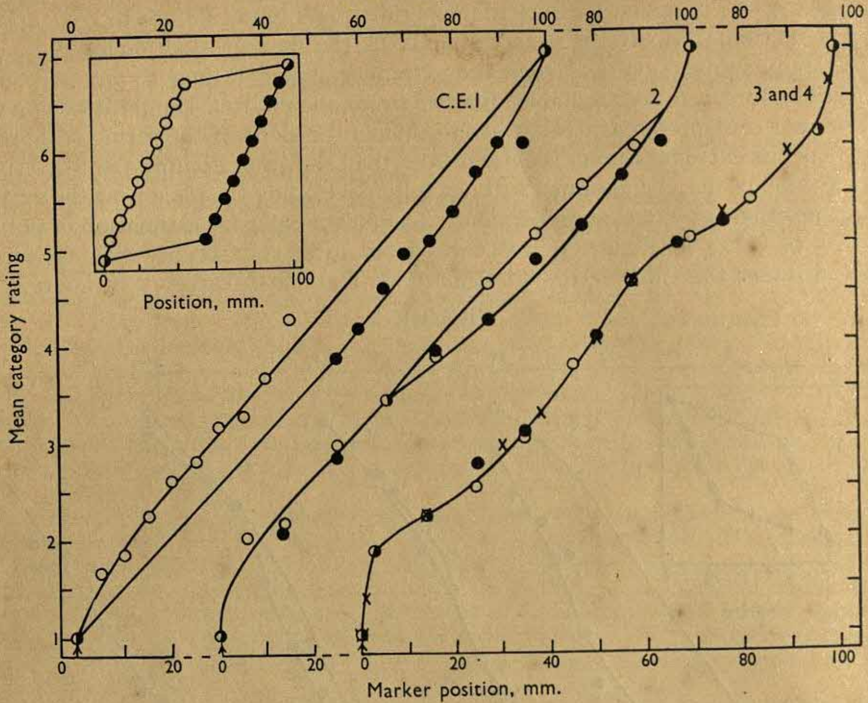
Application of the technique of adjusted spacing to the category estimation of lifted weights. The fourth category estimation was carried out with an independent group of subjects.

based upon the average of the open and filled circles of C.E.3 in Figure 3. Apparently, the resulting category estimation is stable under further iterations.

The results obtained with the category sortings of the three sets of facial photos of Figure 5D are presented in Figure 5A. The corresponding results of the category sortings of the four sets of Figure 5H are presented in Figure 5F. Application of the technique of adjusted spacing upon the results of Figure 5A yielded two new sets of photos. Category sorting of the new sets of photos yielded the results of Figure 5C. Convergence upon a stable category sorting is apparently achieved with the scaling of facial beauty.

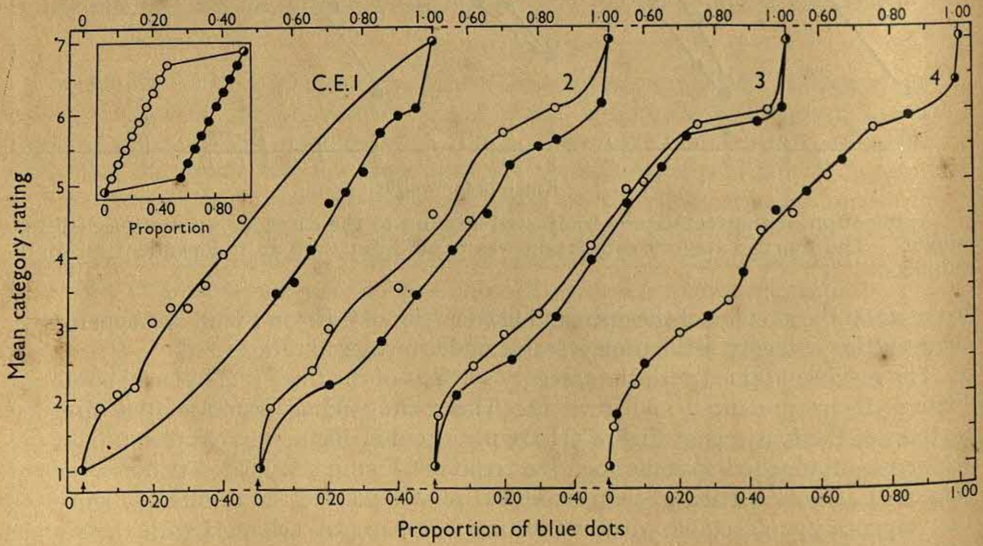
It may be noted that, in the judgement of facial beauty, unlike the case of length, width, relative proportion, and marker position, there is no *a priori* method of ordering

FIGURE 3



Application of the technique of adjusted spacing to the category estimation of the position of a marker along a line of 100 mm.

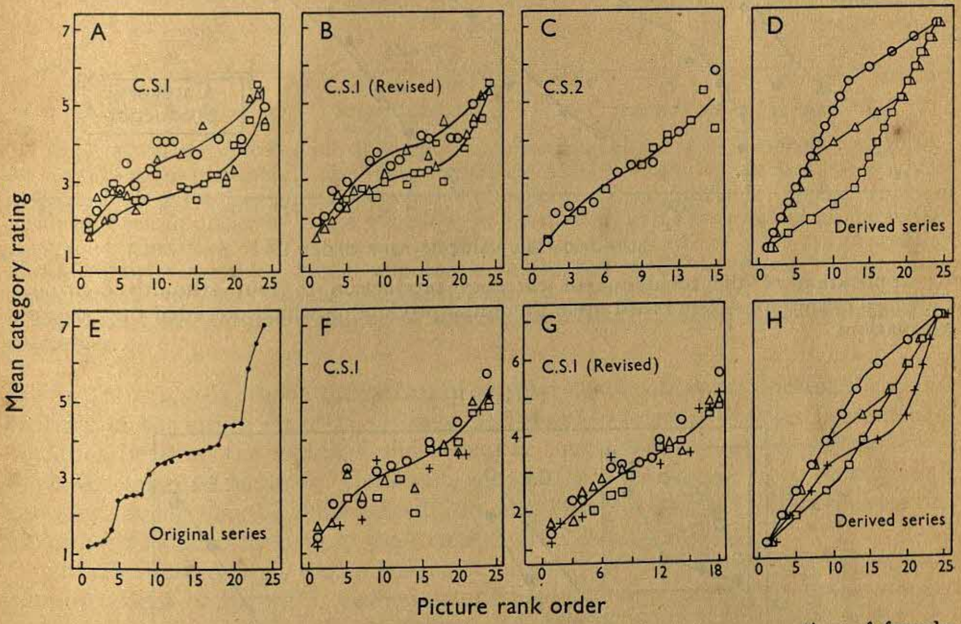
FIGURE 4



Application of the technique of adjusted spacing to the category estimation of the proportion of blue dots out of a total of 100 red and blue dots. The fourth category estimation was carried out with an independent group of subjects.

the stimuli before test. For example, in Figures 5A and 5F, the preliminary rank-ordering was obtained from the absolute judgements, represented in Figure 5E. Recourse to additional test results is unnecessary, since a rank-ordering and a category scaling may both be achieved within the same operation. For example, Figure 5B replots the results of Figure 5A, and Figure 5G replots the results of Figure 5F, with the abscissae modified for minimal inconsistency of rank-ordering within the respective distributions. For example, for three different experiments, assume three pairs of pictures yielded the rank-orderings: a, b; b, d; and c, d. Minimal inconsistency would be obtained with the rank-orderings: a, b, c, d: or a, c, b, d. A comparison of the scores assigned to b and c would resolve the uncertainty.

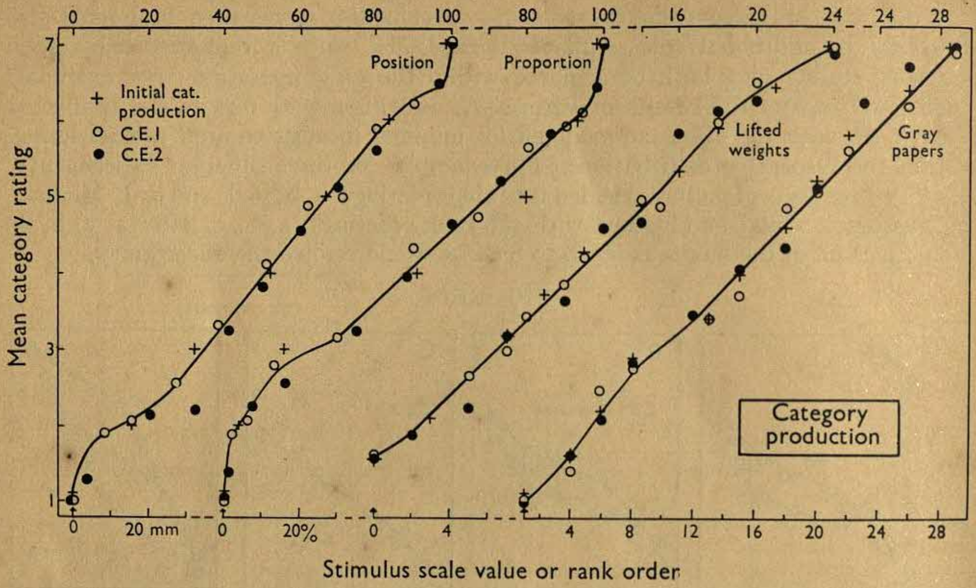
FIGURE 5



Application of the technique of adjusted spacing to the category sorting of female facial profiles. The photographs were originally presented for absolute rating upon a 10-point scale of facial beauty. The relative ratings, in turn, were stretched to a 7-point scale and are presented in E. From the rankings in E, sets of 16 pictures were selected and are represented in D; and sets of 10 pictures were selected and are represented in H. Tests represented in A, B, and C were carried out with the sets of D; tests represented in F and G were carried out with the sets of H. The abscissae of B and G have been adjusted to provide minimal inconsistency of the results of A and F, respectively (see text for details). A separate group was employed for each scaling. The results of D, E, and H are arbitrarily scaled with a maximum ordinate of 7·0; the others are arbitrarily scaled with a maximum ordinate of 6·0.

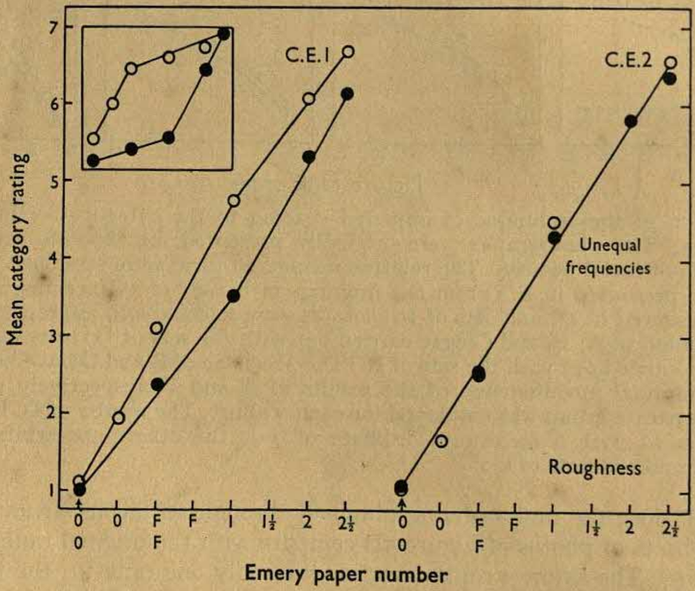
How well does the rank-ordering obtained through an inconsistency analysis of the three subsets of photos of Figure 5D compare with the original rank-ordering of the 24 photos? The average rank deviation was only one rank for the top five and bottom five pictures, but was about three ranks for the entire set of 24 pictures. This average rank deviation is nearly equal to that obtained between the original rank-orderings of the 24 pictures of Figure 5E for the first 20 subjects tested and for the last 20 subjects tested. The 24 pictures were also randomly subdivided into three sets of 10 each with 3 pictures common to each set. The resulting rank-ordering of

FIGURE 6



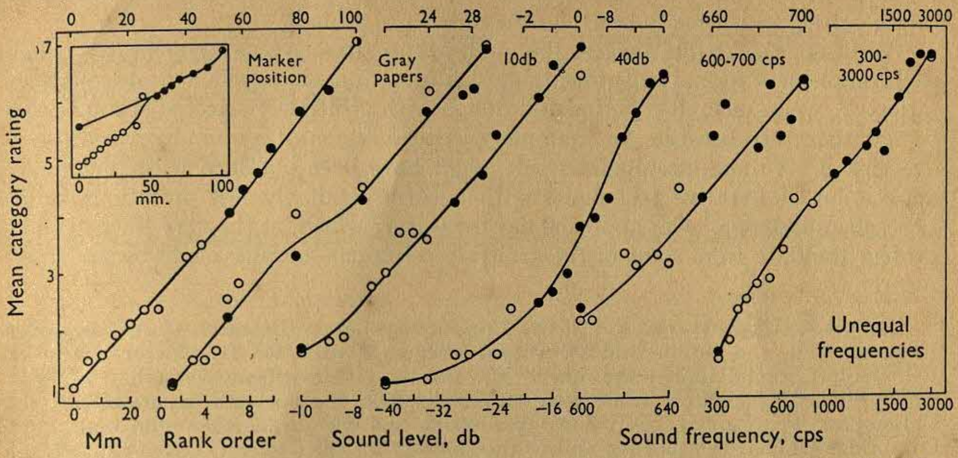
Application of the technique of category production to four stimulus continua. Category estimations were based upon the initial production which preceded the category estimations.

FIGURE 7



Application of the technique of unequal stimulus frequencies to the category estimation of emery papers. The inserts of Figures 7 and 8, unlike the previous inserts, do not represent the initial stimulus sets. Instead, the ordinate of the insert of Figure 7 represents the cumulative relative frequency of presentation on the initial category estimation for emery papers.

FIGURE 8



Application of the technique of unequal stimulus frequencies to the category estimation of four stimulus continua. The insert of Figure 8 applies only to marker position; its ordinate represents the cumulative relative frequency of presentation for marker position resulting from application of the technique of unequal stimulus frequencies to the initial category estimations of two stimulus ensembles of marker position. The initial spacing of the stimuli ensembles, before application of the technique of unequal stimulus frequencies, may be read from the abscissa of Figure 8. For example, with the exception of grey papers, there was no overlap in the intermediate stimuli of each pair of initial stimulus ensembles.

the 24 pictures, based upon the ratings of 12 subjects, closely approximated the rank-ordering of the entire set of 24 pictures carried out in a separate test. The average deviation between the rankings under the two procedures was only 1·5 ranks over the entire range of pictures. The results suggest that convergence upon an unbiased category sorting may be achieved with nominal stimulus scales in which the stimuli are ordered empirically only in the course of the category scaling.

Stimuli derived from the technique of category production, represented by the plus (+) signs of Figure 6, yielded category estimations which were stable under successive applications of the technique of adjusted spacing (C.E.1 and C.E.2 in Figure 6). Moreover, there was little difference in the ratings achieved under the rating and production procedures.

The technique of unequal stimulus frequencies was carried out with a limited number of available emery papers so that interpolation of intermediate papers was not possible. Nevertheless, a stable rating scale was quickly achieved with emery papers (Figure 7) as well as with other stimulus variables where interpolation was possible under the technique of adjusted spacing (Figure 8).

DISCUSSION

Results obtained with additional stimulus variables are in agreement with the present findings. Convergence upon an unbiased category estimation is quickly achieved with a wide variety of techniques and with a wide range of stimulus variables. Although the necessary and sufficient conditions for convergence upon an unbiased category estimation have not been defined, it appears that only one condition must be obtained: the discrepancy between the initial category estimations (C.E.1 for open vs. filled dots) should be less than the corresponding discrepancy between the rank-orderings of the stimulus ensembles (insert graphs of Figures 1–5 for open vs. filled dots). This condition tends to be easily satisfied because the number of rating categories is usually less than the number of stimuli.

All in all, the results suggest that well-defined iterative experimentation might well complement formal experimental design procedures in psychological experimentation. A well-executed experimental design with arbitrarily selected stimuli might be unsatisfactory because, for example, a critical region of a stimulus variable was overlooked. A more meaningful result might have been obtained with preliminary application of iterative techniques with a correspondingly less ambitious formal experimental design. This approach has the feature which, at the very least, tends to protect the data from some of the arbitrary selections initially made by an experimenter.

The research was carried out at the Applied Psychology Research Unit, Cambridge, England, while the author held a National Sciences Foundation Postdoctoral Research Fellowship. Special thanks are due to Mr. Donald E. Broadbent, Director, A.P.R.U., who provided a research atmosphere unparalleled in the author's experience, to Dr. Christopher Poulton for many long discussions and for arrangements in testing the Cambridge University students, and to the subjects for their tolerance to a brash Yank. The facial photographs were kindly supplied by Mr. Iliffe of the University of Keele, as part of his comprehensive study of the determinants of facial beauty.

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COMPARATIVE ANALYSIS OF PROPRIOCEPTION IN LEFT AND RIGHT ARMS*

BY

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Using the "kinaesthetic memory for the target" technique, differences in the accuracy of pointing to a target with the right and left arms are analysed. The effect of rotation of the head to left and right upon this process is also studied.

With the head normally orientated, it was found that pointing with the right arm is significantly better than with the left. Accuracy of pointing is greater with the target directly in front of the body than when it lies to either left or right side.

When the head is rotated, the direction of the pointing error is inversely related to the direction of rotation.

The study suggests that the precision of control over arm (in the absence of vision) is related to the varying ability of individual subjects to correlate limb movements with the prevailing orientation of the body, especially of the head and neck. This is additional to the influences of genetically-determined handedness and of the sensory input from the moving limb.

The precision with which subjects can point to a target in space without the coincident aid of vision has been analysed by a number of workers. It is difficult to summarize their findings as the results vary with the particular test procedure employed.

Some of the experiments have been performed with preliminary visual training (Fleishman, 1958), testing a kind of "kinaesthetic memory for the target." Other studies have deliberately excluded visual training, the blindfolded subject being asked to re-indicate a target to which his hand has previously been directed—actively by the subject himself searching for the target without the aid of vision (Cohen, 1958), or passively by the experimenter manipulating the hand of the subject to the required position (Merton, 1961). Other analyses have been carried out by measuring the accuracy of pointing to the target when only the target but not the hand can be seen (Sandstrom, 1951; Szafran, 1951; Whiteside, 1961). In none of these experiments, however, has there been any attempt to establish whether a subject is able to point with greater accuracy with one arm rather than the other. Although Szafran (1951) tested both arms in his subjects, he did not make a comparative analysis of the performance by the two hands, on the grounds that the right and left arms followed the "same patterning of reactions."

This latter assumption is also implicit in some of the conventional procedures employed by clinicians in examination of the sensory system in limbs. Thus, the traditional procedures requiring the blind-folded patient to reproduce with one limb a posture into which the contralateral limb has been manipulated; to maintain bilaterally symmetrical static postures of homologous limbs; to perform pointing tests, first with one limb and then with the other; and to identify passive movements at homologous joints on the two sides with equal precision, all presuppose a bilaterally equal degree of precision in the ability to move or to hold steady the two limbs.

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* This work was conducted at the Psychological Laboratory, Institute of Neurology, (Queen Square).

This report describes some experiments designed to analyse the differences in pointing to a target with the right and left arms of normal adult subjects using the "kinaesthetic memory for the target" technique. Subsidiary studies also reported here include a comparison of the accuracy in pointing to a target with the two arms when the head is rotated to left and right. In addition the results are analysed in order to compare the accuracy of pointing to a target situated in the right, central or left sectors of perceptual space, and also in relation to movements performed in ipsilateral and contralateral directions with either arm.

METHOD

Subjects

The subjects were 24 normal adults (men and women) between 18 and 35 years of age. All but two of them were right-handed.

Apparatus

This was a modification of the Fleishman position-finding apparatus (1958) and consisted of a semicircular arrangement of 57 pegs (see Figure 1) mounted on a board.

FIGURE 1

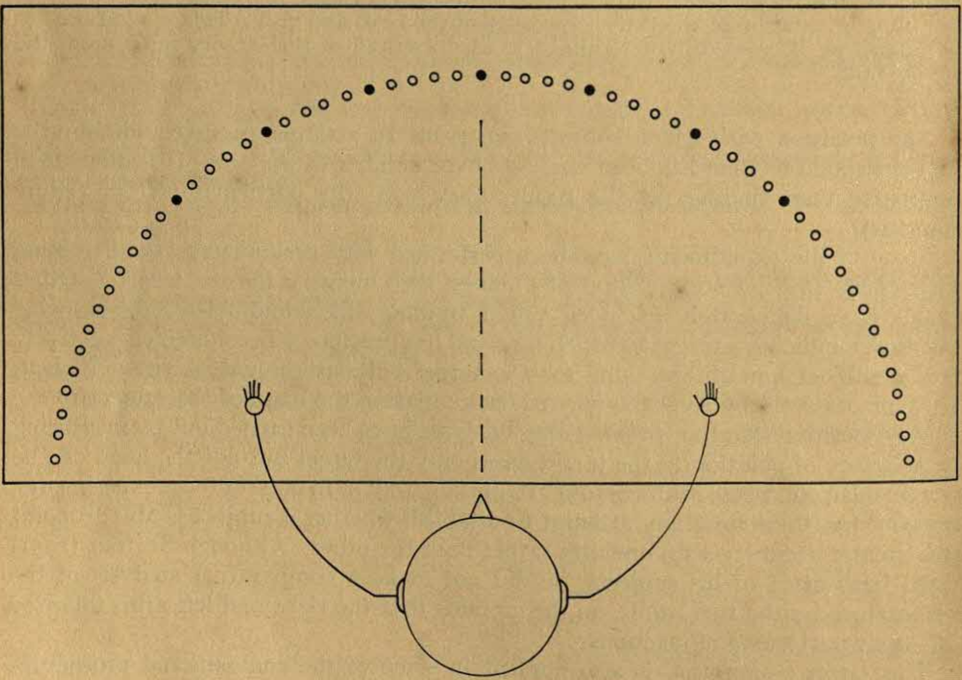


Diagram illustrating the apparatus employed. The black dots indicate the various positions in which the black pegs were placed. (Description in text.)

The transverse distance between the pegs at the extreme right and left ends of the arc was 91 cm. The angular separation between each pair of pegs at the vertical axis of the subject's body was 3·2 degrees. Under these conditions, all pegs were within easy reach at a constant distance of 45·5 cm. (measured from the midline of the body). All the pegs were white, except for three black pegs. One of these black pegs was located at the centre of the arc, while the other two were placed in varying positions along the right and left halves of the arc, but always with the left and right black pegs at the same angle from the subject's vertical body axis.

Procedure

The subject first inspected the arrangement of the pegs and was shown the starting position, this being the centre of the edge of the peg-board. He was allowed 15 preliminary trials in pointing to each of the black pegs with the use of vision. The subject's eyes were then masked, and he was asked to touch with his forefinger either the centre, left or right black pegs and hold his finger (as soon as he touched a peg) in that position until the necessary measurements were made. He was instructed to return his hand to rest at the starting position after each attempt. The order of presentation of left, right and centre pegs was fixed for each subject, the sequence having been planned randomly.

The tests with either arm were carried out with the head in each of three positions—

(1) Head erect, with the sagittal plane directed straight forward along the central radius of the board.

(2) After the blindfolded subject had turned his head horizontally to the left through 90 degrees.

(3) After the blindfolded subject had turned his head horizontally to the right through 90 degrees.

Care was taken to see that in no case was there any lateral head tilt. In positions (2) and (3) the preliminary practice trials were performed with the head directed straight forwards, and not with the head rotated laterally.

In order to avoid the effect of learning when changing from one to another of the above head positions, the location of the two lateral black pegs was altered on each occasion. Thus these pegs were placed to subtend angles of 16, 34 and 48 degrees at the subject's body. In all positions the black pegs were symmetrically located in the right and left quadrants of the peg-board. There were thus three head positions to be tested, and three patterns of the black pegs; and the subjects were arranged in three groups of eight each, so that each group of eight subjects was tested with all three peg patterns in one of the three head positions.

Pointing was tested with the left and right arms in every subject. Twelve of the subjects performed all the tests first with the right arm and then with the left arm; the other twelve subjects carried out the tests in reverse sequence. The choice of whether an individual should start with left or right arm was randomly varied. Comparable head positions and peg patterns were employed for the right and left arms in every case.

Analysis of Results

Test results were scored as cumulative errors, based on the number of pegs through which the subject's index finger deviated to right and left of the designated peg at each attempt in fifteen trials in every experimental combination of head position and peg pattern. Thus a numerically high score indicates a poor performance, and *vice versa*. In presenting the results, no distinction is made between the scores in the three peg patterns, as there was no statistically significant difference between them. By choosing cumulative errors rather than the average of 15 trials, an attempt is made to emphasize that the results represent the relative precision with which the subjects can point to a target, rather than an accurate measurement of the amount of error.

Three main comparisons are being made

(i) Differences when using the left and right arms.

(ii) Differences when pointing to the centre, to the left and to the right sides of perceptual space.

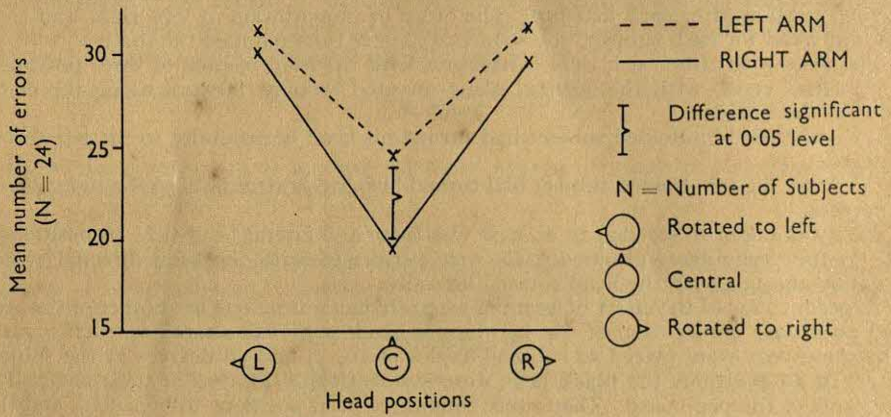
(iii) Differences between ipsilaterally and contralaterally directed movements.

RESULTS

(i) *Comparison when using the left and right arm*

The mean of the cumulative errors obtained with the left and right arms in the three conditions of the experiment are shown in Figure 2. The cumulative errors, for the 15 trials, when using the right arm (head being central) was 19·21 (S.D. 8·92). This is significantly smaller than the errors made when using the left arm (24·18, S.D. 12·58). Test for related means shows the difference between the scores for the left and right arm to be significant at the 0·05 level. With the head rotated to the left the number of errors for the right arm was 29·54 (S.D. 17·03) and the errors for the left arm 30·98 (S.D. 14·47)—that is to say, there is a better performance with the right arm than with the left (see Table I), but this difference is not statistically significant. Likewise with the head rotated to the right, the number of errors with the right arm was smaller (29·12, S.D. 10·20) than with the left arm (30·64, S.D. 14·67). However this difference is not statistically significant (see Figure 2).

FIGURE 2

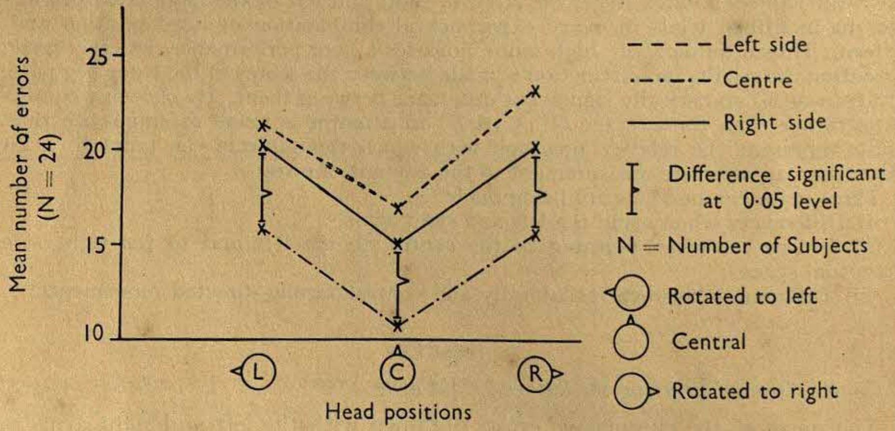


Comparison between left and right arms.

(ii) *Differences when pointing to the centre, to the left and to the right sides of perceptual space*

These results were obtained by comparing the accuracy of pointing (irrespective of the hand used) to the centre, to the left and to the right of space. The mean score (shown in Figure 3) is the cumulative errors (see Table I) for ten trials. In

FIGURE 3



Comparison between pointing to the centre, left and right of perceptual space.

each of the three positions, the error was larger when the arm movement was directed to the left side of the body and least when the arm movement was in the central plane of the body. Tests for related means show that there is a significant difference (at the 0·05 level) between errors made in the right side of space and those made with centrally directed movements. However, there was no statistically significant

TABLE I

Means of Cumulative Pointing Errors for the three Head Positions and Relevant Comparisons

Head position	*Left*						*Central*						*Right*					
(i) Arms ..	Left			Right			Left			Right			Left			Right		
(ii) Space ..	L	C	R	L	C	R	L	C	R	L	C	R	L	C	R	L	C	R
(iii) Movements ..	I		Co	Co		I	I		Co	Co		I	I		Co	Co		I
Deviation to the left	4·91	2·20	3·67	4·16	1·70	1·96	7·80	1·86	4·50	5·38	3·25	3·10	9·20	5·76	6·58	7·50	6·29	7·20
Deviation to the .. right	6·91	6·63	6·66	6·04	5·92	9·66	2·44	3·52	4·00	1·75	1·96	3·77	3·75	1·79	3·56	3·33	1·79	3·01
Total deviation ..	11·82	8·83	10·33	10·20	7·72	11·62	10·24	5·38	8·50	7·13	5·21	6·83	12·95	7·55	10·14	10·83	8·08	10·21

L = left side of space; C = central space; R = right side of space; I = ipsilateral movement; Co = contralateral movement. The number in each cell represents the cumulative errors in 5 trials.

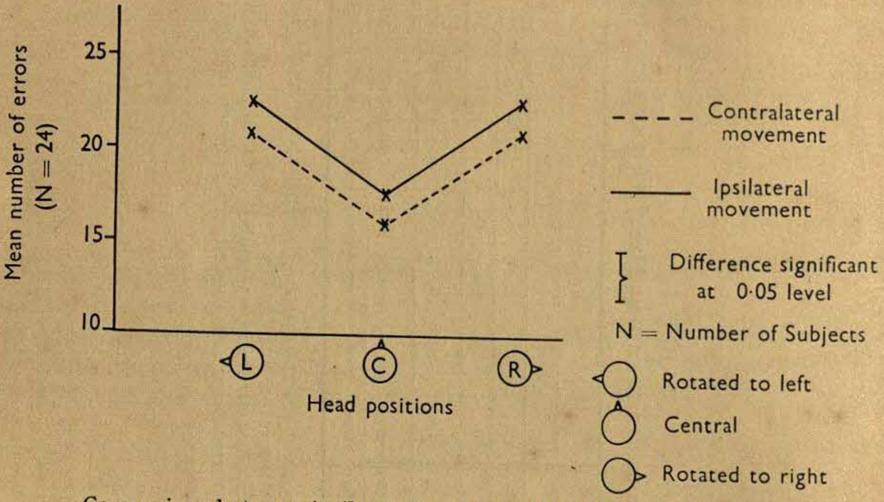
(i) Comparison between arms is based on cumulative errors in 15 trials. (ii) Comparison when pointing to different parts of perceptual space, and (iii) between ipsilateral and contralateral arm movements is based on cumulative errors in 10 trials. See text.

difference between the errors made when pointing to the left and to the right sides of space (see Figure 3).

(iii) *Differences between ipsilaterally and contralaterally directed arm movements*

These results were obtained by pooling all the errors made by the left and right arms when moving to the same side of the body (i.e. ipsilaterally), and all the errors made by the left and right arms when moving across the body (i.e. contralaterally). Errors in movements toward the central peg were omitted in this case, so that the results are based on the cumulative errors in ten trials. It will be seen (Table I) that on the whole there is a poorer performance when making ipsilateral movements, than with contralaterally directed movements. This difference, however, is not statistically significant (see Figure 4).

FIGURE 4



Comparison between ipsilateral and contralateral movements.

(iv) *Direction of the error when pointing to a target*

Analysis of the direction of the errors (see Table I) shows that with the head directed centrally there is no significant difference between the number of deviations to the right and to the left of the target peg. The cumulative errors of deviation to the left was 25·89 and the cumulative errors of deviation to the right was 17·44. The value for this comparison is not statistically significant.

With the head rotated to the left or to the right, there are significant differences in the number of errors. With the head rotated to the left, the cumulative errors of deviation to the left are 18·60, and 41·82 to the right. This difference is significant at the 0·01 level of confidence. With the head rotated towards the right, the cumulative errors of deviation towards the left are 42·53 and 17·23 with deviation towards the right. The t value for the difference between these two figures is significant at the 0·001 level of confidence.

DISCUSSION

This study shows that when arm pointing tests are conducted with the head centrally orientated in relation to the body, performance with the right arm is significantly better than with the left arm. However, horizontal rotation of the head to right or left renders this difference in accuracy between right and left arms no longer significant. Furthermore—and irrespective of which arm is used—accuracy is greater when pointing to a target located directly in front of the body than when pointing either to the right or to the left of this spatial plane. Pointing to a target located on the right side of the body appears to be more accurate than pointing to a left-sided target—although the difference is not statistically significant. If pointing movements be made across the midline of the body (i.e. contralaterally), they appear to be more precisely directed than similar movements made on the same ipsilateral side of the body—but again the difference is not statistically significant. When the head is straight, the errors occur randomly to either side of the target; but with the head turned to the left the errors are consistently directed towards the right side, and *vice versa.* In other words, errors are directed to the side of space opposite to that to which the head is turned.

In the conditions imposed by the present experiment, the ability to point to a target without the aid of vision is determined by the following factors:

(*a*) Visual memory of the position of the target in space.

(*b*) Precision of motor control of limb movement.

(*c*) Sensory information from the tested limb.

(*d*) Supplementary sensory information from receptors elsewhere in the body—such as the labyrinthine receptors, the receptors responsible for neck reflexes, and cutaneous receptors.

In order to evaluate the significance of the different results obtained with the left and right arms, it would be desirable to be able to determine the relative contribution of each of the above factors in the prevailing experimental situation.

Cohen (1958) has suggested that the phenomena studied in experiments of this type represent a combination of "visual proprioception, arm proprioception and the ability to associate these two." It is outside the scope of this paper to consider the possible role of visual proprioception; it is sufficient to say that the visual information involved in this test is similar for both arms, so that visual memory of the target location is unlikely to bias the performance in favour of one arm rather than the other.

Regarding the precision of motor control of limb movement, it might be suggested that movements of the arm controlled from the dominant cerebral hemisphere are likely to be more precise than those of the opposite limb. However, this does not appear to be the crucial factor underlying the differences between the arms observed here for the following reasons. First, accuracy of pointing with the right hand was not significantly greater than that with the left in the preliminary trials. Second, the nature of the task to be performed did not demand a degree of skill such that the performance with the right arm was consistently better than that with the left arm.

It seems, therefore, that the most important factor contributing to the differences in accuracy of pointing to a target without the aid of vision, in the present experiments, is the sensory information from the moving limb. If this be so, it is necessary to consider whether the difference between left and right arms might depend upon differences in receptor density in the two arms, or whether it is determined by central mechanisms responsible for the integration of sensory inputs from the arms with the sensory information relating to the position of the body and head.

There is no neurohistological evidence of differences in the density of receptor populations in the two arms. Further, were such factors the only explanation of the differences of pointing accuracy between the left and right arms, it would be expected that the differences in the efficiency of the performance on the two sides would obtain in all circumstances of the experiment—yet the difference is abolished by rotation of the head.

The findings reported here suggest that the mechanisms responsible for the differences in the two arms involve integration of control of arm movement with head position. When the head is centrally orientated, the influence of past experience and practice renders right-handed individuals more accurate with the right arm than with the left. When the head is rotated to one or other side of the body, it may be that with the change of posture the information from neck proprioceptors may distort past sensory experience (i.e. the experience acquired by constant reaching and grasping with the preferred hand) so that the right hand no longer shows the degree of accuracy displayed when the head is centrally orientated. It seems, then, that the precision of control over movements of an individual limb (in the absence of vision) does not depend solely upon genetically-determined handedness, nor solely upon the sensory input from the limb, but also upon the varying ability of individual subjects to correlate limb movements with the prevailing orientation of the body in space—and particularly with the posture of the head and neck.

That the accuracy of pointing to a target with one arm is influenced by sensory impulses from the neck is supported by the supplementary observations reported in this paper. For example, the precision of movements in the central plane is greater than when the movements are made to either side of it. This suggests that the accuracy of movements in space may be dependent upon the spatial location of the target in relation to the head and neck. Further, in the head-turning experiments the learning trials were performed with the head in the central position, while the tests were done after the head had been rotated to right or left. If the subject's knowledge of the position of the target were derived only from the impulses evoked from the moving arm, the subsequent rotation of the head should not affect the results significantly. Yet, as can be seen in Table I, the average error was greater after the head was turned to either side than when it was in the central position. It is clear, therefore, that the subjective location of the target in the absence of vision is affected by altering the position of the head in relation to the target.

One further aspect of the present findings requires comment; namely, the consistent inverse relation between the direction of rotation of the head and the direction of the pointing error. This might be regarded as some form of "overcompensation," and was found consistently in all subjects. Such overcompensation is significantly more marked if the head be turned to the right, for the erroneous deviation of the limb towards the left is then very marked. A similar overcompensation error occurs in the past-pointing associated with dynamic rotation of the head, and the consequent asymmetry of labyrinthine proprioceptive inputs (Brodal, Pompeiano and Walberg, 1962; Dow and Moruzzi, 1958). Thus it may be that the two phenomena are in some way related to one another.

The observations reported here have important practical implications for the choice of methods for the clinical testing of sensory function. The necessity for paying attention to the position of the head and body when performing such tests is apparent from the results described; and failure to do so may lead to diagnostic errors in the evaluation of disorders of sensation.

I should like to thank Dr. E. A. Carmichael and Dr. R. T. C. Pratt for their advice, and Dr. A. R. Jonckheere for statistical help. I am also grateful to Mr. A. H. Prickett of the Department of Medical Photography of the National Hospital, for the photographs. The work was supported by a Medical Research Council grant.

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SHORTER ARTICLES AND NOTES

ACQUISITION AND EXTINCTION OF A SIMPLE MOTOR SKILL AS A FUNCTION OF DELAY OF KNOWLEDGE OF RESULTS

BY

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From the Texas Christian University

The present experiment demonstrates that the effects of delay of knowledge of results (KR) in a line drawing task depend upon the dependent variable which is considered and the original response tendency of the subjects. Delay of KR interfered with the acquisition of the correct response when number of correct responses is the dependent variable. When KR was omitted the immediate-KR group continued to make more correct responses than the delayed-KR group. However, there was a significant reduction in correct responses for both groups. When absolute error was the response measure there were no significant differences between immediate- and delayed-KR groups either during acquisition or extinction. Analysis of the type of response made during extinction suggested that the overshooting effect obtained by previous investigators may be typical of short responders trained under conditions of immediate reinforcement but not of those trained under delayed-KR.

Greenspoon and Foreman (1956) obtained clear cut evidence that the effectiveness of knowledge of results (KR) in a line drawing task varies inversely with the time delay between the response and KR. On the other hand, Bilodeau and Bilodeau (1958) were unable to demonstrate a similar effect in five different experiments. Bilodeau and Ryan (1960) hypothesized that the Greenspoon and Foreman findings could not be replicated and they did in fact obtain null results. However, Dyal (1965) has recently replicated the Greenspoon and Foreman results for the case of a 30 sec. delay interval.

The purpose of the present experiment is twofold: (*a*) to provide replication of Greenspoon and Foreman's study at the 20 sec. delay interval used by Bilodeau and Ryan, and (*b*) to determine the effect of elimination of KR on a simple motor response which has been formed on the basis of KR.

METHOD

Subjects and Experiments. The subjects were 54 undergraduates at Texas Christian University. The experimenters were 18 students enrolled in the course in Experimental Psychology. Each experimenter selected three subjects from a list of 60 volunteers, and assigned them at random to the three experimental conditions, with the restriction that no experimenter trained a subject whom he knew.

Apparatus. The apparatus was patterned after that of Greenspoon and Foreman. It consisted of a 2 ft. by 2 ft. piece of plexiglass mounted on a small table. A slot 12 in. by ⅛ in. was cut through the plexiglass in the centre. An L-shaped metal strip 13 in. long was mounted along the slot. Lines indicating the "correct" zone were marked off 2¾ in. and 3¼ in. from the left stop of the metal strip. The Greenspoon and Foreman apparatus was modified so that the subject drew his line on a plastic surface underneath the slot rather than on wrapping paper. The metal strip was marked off in ⅛ in. units on either side of the "correct zone" so that absolute error measures could be obtained.

In order to improve the riding qualities of the instrument an empty Lindy Pen was substituted for the pencil. It was provided with a tape finger grip similar to that used by Bilodeau and Ryan.

Procedure. After being seated in front of the apparatus the subject was read the following instructions:

"What I want you to do is to draw 3 in. straight lines horizontally from left to right while blindfolded like this (experimenter demonstrated by drawing a horizontal line in the air). When you have completed your line, leave your hand at the point where you have stopped. I shall return your hand to the starting point and a little later I shall give you a signal to draw the next line by saying 'draw.' Keep your arm and hand off the table while drawing your line. You are to have only the point of the pen touching the surface. However, you may rest your arm on the table after you have drawn each line. Are there any questions?"

Questions were answered by repeating the pertinent part of the instructions.

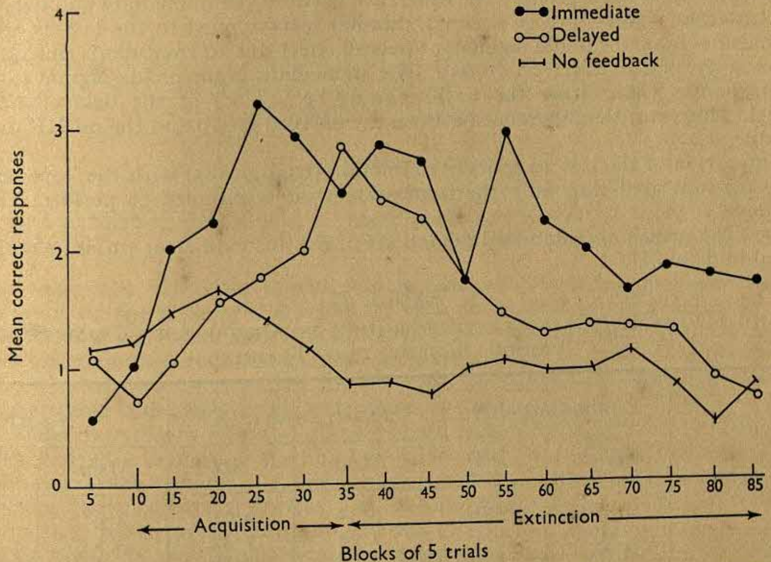
The two experimental conditions were immediate-KR (approximately 1·0 sec. delay) in which the subject was given KR immediately after completion of his response, and delayed-KR (20 sec. delay) in which the subject was given KR information 20 seconds after completing his response. Twenty-five seconds after completion of the response the subject's hand was returned to the starting point and five seconds later he was given the signal "Draw." The procedure was the same for the control group except no KR was given.

Each subject drew 85 lines. The first ten trials no KR was given to any subject. Trials 11–35 constituted the acquisition period during which KR was given to the experimental subjects. During trials 36–85 KR was eliminated for the experimental subjects. All other procedural details were the same as in the previous experiment by Dyal (1965).

RESULTS

Correct responses. The mean number of correct responses in blocks of five trials is presented in Figure 1. The means and standard deviations for selected segments are presented in Table I. Analysis of variance applied to the data for trials 1–10 indicated that the groups were not significantly different prior to the introduction of KR.

FIGURE 1



Mean number of correct responses during pre-training (trials 1–10), acquisition (trials 11–35), and extinction (trials 36–85). Each point represents the total number of correct responses for all subjects over a 5-trial block, divided by the number of subjects.

Simple analysis of variance applied to the mean number of correct responses during trials 11–35 resulted in $F = 10{\cdot}16$; d.f. = 2,51; $p < 0{\cdot}01$. Duncan's multiple range test revealed that the immediate-KR group made significantly more correct responses than the delayed-KR group ($p < 0{\cdot}05$) or the no-KR group ($p < 0{\cdot}001$). The delayed-KR group made more correct responses than the no-KR group ($p = 0{\cdot}05$).

It should also be noted that, although the immediate group made significantly more correct responses throughout acquisition than did the delay group, there is no reliable difference between the two groups at the end of acquisition (trials 26–35).

Correlated t-tests applied to the difference in number of correct responses from trials

TABLE I

Mean and SD of Number of Correct Responses per 5-Trial Block Over Various Segments of Training

Trials	*Immediate-KR*		20 *sec. Delayed-KR*		*No-KR*	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*
1–10	0·78	0·84	0·92	1·00	1·17	1·20
11–35	2·56	0·70	1·84	0·59	1·29	1·15
36–60	2·32	0·97	1·78	0·87	1·01	0·97
61–85	1·81	1·38	1·17	0·90	0·90	0·91

1–10, during which no KR was given, and the last 10 trials of acquisition (trials 26–35) indicated that both the immediate and the delayed groups significantly increased the number of correct responses ($p < 0·01$). There was no reliable difference in the case of the no-KR group.

For purposes of analysis, the extinction data were divided into two phases of 25 trials each. Simple analysis of variance applied to the mean number of correct responses trials 36–60 resulted in $F = 6·62$; d.f. = 2,51; $p < 0·01$. Duncan's multiple range test revealed that both immediate and delayed groups continued to make significantly more correct responses than the no-KR group. The difference between the immediate and delay groups was of borderline significance ($p = 0·05$). Similar tests applied to the last 25 extinction trials (trials 61–85) indicated a significant overall effect due to treatments during acquisition ($F = 3·37$; d.f. = 2,51; $p < 0·05$). The immediate group made significantly more correct responses than either the no-KR group ($p < 0·01$) or the delayed-KR group ($p < 0·05$). However, the difference between the delayed group and the no-KR group was not reliable.

A comparison of the last 10 trials of extinction (trials 76–85) with the "operant" level (trials 1–10) indicated that only the immediate group continued to perform above the operant level.

Errors. The means and standard deviations of the absolute error (in units of $\frac{1}{8}$ in.) are presented in Table II.

TABLE II

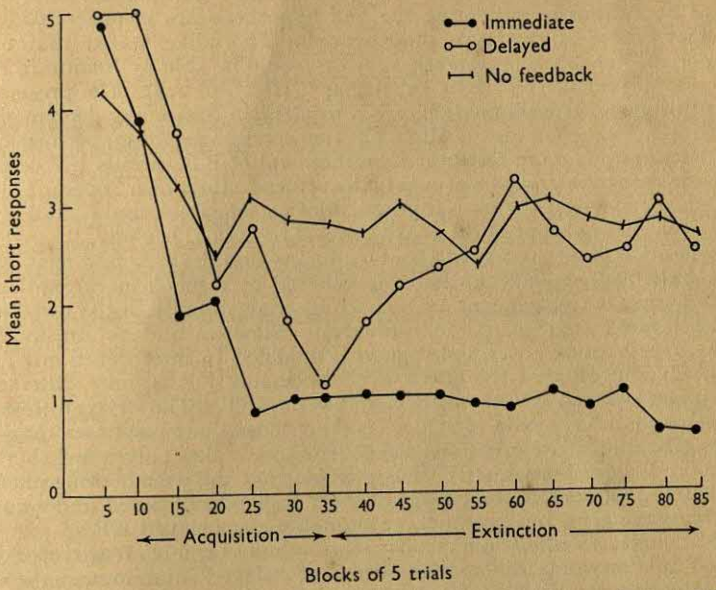
Mean and SD of Mean Absolute Error (in $\frac{1}{8}$ in. Units) per 5-Trial Block Over Various Segements of Training

Trials	*Immediate-KR*		20 *sec. Delayed-KR*		*No-KR*	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*
1–10	4·20	2·61	5·32	4·01	4·08	3·47
11–35	1·46	1·41	1·94	1·22	3·13	1·92
36–60	1·65	1·45	1·68	0·84	3·32	1·80
61–85	2·45	2·26	2·49	1·24	3·46	2·88

Analysis of variance applied to the mean absolute error data for trials 1–10 required acceptance of the null hypothesis. A similar analysis applied to the mean absolute error during trials 11–35 resulted in a significant F value of 4·67 (d.f. = 2,51; $p < 0·05$). Comparison of the subgroups by Duncan's range test revealed that the immediate and delayed feedback groups made significantly less errors than the no-feedback control group. However, the performances of the immediate- and delay-groups were not reliably different. Exactly the same conclusions were supported for the first half of extinction, whereas the performances of the three groups during the last half of extinction were not significantly different.

Comparison of the performance of the separate groups at the end of acquisition (trials 26–35) with their own "operant" level, revealed that both the immediate- and the delayed groups made significantly less errors. A similar comparison based on the performance at the end of extinction (trials 76–85) revealed that only the delayed group was still making less error than its "operant" level. However, it was apparent from graphic representation of the data that this effect was due to the higher "operant" level of the delayed group rather than an especially low error at the end of extinction.

FIGURE 2



Mean number of short responses during pre-training (trials 1–10), acquisition (trials 11–35), and extinction (trials 36–85). Each point represents the total number of short responses for all subjects over a 5-trial block, divided by the number of subjects.

Short responders. In order to describe the present data more precisely, the subjects were grouped on the basis of the dominant type of error which they made during trials 1–10. As is commonly the case (Thorndike, 1927; Trowbridge and Cason, 1932; McPherson, Dees and Grindley, 1948) the majority of the subjects whose initial response tendency was clear, tended to undershoot, i.e. they were short responders. Inspection of Figure 2 reveals a striking difference during extinction between those short responders who were trained under immediate-KR and those trained under delayed-KR. The delayed-KR group tends rapidly to increase the number of *short* responses, whereas the immediate-KR group shows no such tendency. A similar figure of the number of *long* responses revealed the opposite tendencies to be present, i.e. the immediate-KR group tends to make *more long* errors than the delayed-KR group. Because of the small number involved statistical analysis of this data was not particularly meaningful and at this time the relationship must be considered to be only suggestive.*

DISCUSSION

When mean number of correct responses is the dependent variable, the present results are in agreement with the previous experiment by Dyal (1965) and support the empirical generalization that delay of KR interferes with acquisition of a simple motor skill. This conclusion is in accord with the results reported by Greenspoon and Foreman (1956) and opposed to the results of Bilodeau and Ryan (1960). However, it should be noted that the effect of delay of KR was not as large as that reported by Greenspoon and Foreman. In fact, by the end of acquisition the performances of the immediate and the delayed groups were essentially the same.

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* Because of the highly suggestive quality of the relationships revealed in Figure 2, further investigation of this phenomenon was undertaken through the M.A. thesis of Mrs. Gene Arrington. Analysis of her data, based on large *N*s supports the differences between immediate and delayed KR during extinction at a statistically significant level.

When absolute error during acquisition is the dependent variable no significant difference is obtained between the immediate and the delay conditions. Although the differences early in training are in the expected direction, by the end of training the performance of the two groups is roughly the same. The absolute error data are thus consistent with the conclusion of Bilodeau and Ryan that delay of KR does not interfere with acquisition of a simple motor skill. However, the usual cautions regarding the acceptance of an experimental hypothesis by accepting the null hypothesis are in order here.

In the first report of this type of experiment Thorndike stated that having once acquired a correct line drawing response subjects would be able to "maintain their ability without incessant guidance by 'right' or 'wrong' " (1927, p. 222). The present results do not support this view. The elimination of KR results in a continuing decrement in correct responses as practice continues. Although the correct response performance of the immediate-KR group is more resistant to withdrawal of KR than is the delayed group, it appears clear that both groups will eventually return to their own "operant" level and/or to the performance level of the no-KR group. McPherson, Dees, and Grindley obtained a similar but even more abrupt effect of withdrawal of KR. They say, ". . . there was an abrupt deterioration to an average of about 0·4 in. even on the very first trial. This abrupt change was statistically significant and was followed by a much more gradual fall off in performance during the subsequent 40 N (no-KR) trials, usually in the direction of overshooting the target" (1948–9, p. 70). Bilodeau, Bilodeau and Schumsky (1959) also obtained the overshooting effect and suggest a modified I_R interpretation. The present data tentatively suggest that the effect of discontinuing KR is quite different for *short* responders under delayed-KR than under immediate-KR. The delayed group tends to revert to their original response tendency, making more short responses than the immediate group. The immediate group, on the other hand, makes only a few short responses but many more long responses. It is thus possible that the overshooting effect reported by previous investigators may be typical of short responders trained under conditions of immediate reinforcement, but not of those trained under delayed-KR.

Since most subjects *initially* undershoot there were not enough *long* responders (initial overshooters) to determine differential effects of delayed- and immediate-KR during extinction. However, it would be predicted that *long* responding subjects trained under *immediate* KR would make more *short* errors during extinction than subjects trained under delayed-KR. On the other hand, the *delayed-KR group* should make more *long* errors during extinction than the immediate-KR group.*

This experiment was conducted as a laboratory problem by students in an advanced undergraduate and graduate level course in Experimental Psychology taught by the first author. The second and third authors supervised data collection and performed the data analysis. Both the present data and previous research by the first author (Dyal, 1965) indicate that experiments involving multiple experimenters can obtain reliable data provided they are adequately trained and supervised.

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* Mrs. Arrington's data, although still based on a small number of long responders, supports the prediction.

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VISUAL DISCRIMINATION OF SHAPE BY HUMANS. II:

(i) matrix figures and (ii) minimal area changes.

BY

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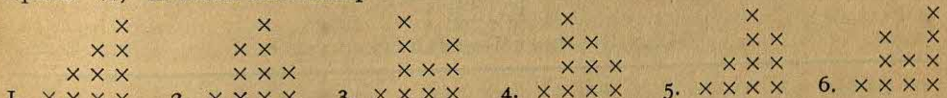
Two experiments are described in which an attempt was made systematically to vary two dimensional shapes according to a pre-arranged design. In the first, subjects were presented tachistoscopically with pairs of "reflexive matrix figures" whose members were either horizontally or vertically orientated; and it was found that reaction times to horizontal pairs were faster than to vertical ones, a result that is in keeping with previous findings. In the second experiment two ensembles were devised that were alike in every respect save that one group was extended or reduced vertically while the other varied horizontally. Performance was better on the vertically orientated ensemble. These findings are briefly related to former studies and the pre-eminence of vertical symmetry is underlined.

INTRODUCTION

In an earlier paper (Butler, 1964) two methods of studying shape discrimination by human subjects were described, the first using a modified Dodge tachistoscope, the second being a card-sorting procedure. When the shapes displayed were common to both methods, the results were identical. The work reported here is the continuation of the project, which makes three assumptions:—(*a*) that, by these methods, only one dimension is under analysis, i.e. that all shapes are translated into a finite amount of excitation; (*b*) that shapes would thus be rangeable along a single continuum. (This assumption seems justified when one considers that mensuration demands the emphasis of one characteristic to the exclusion of others: thus, weighing ignores length, while the measurement of length is not concerned with weight). And (*c*) that a long response time indicates a "small difference" between the stimuli, while a brief response time signifies a "large difference."

EXPERIMENT I

The shapes used in the previous paper were taken from a report by Sutherland (1957) to facilitate comparisons between the two sets of results. Here, for the first experiment, the stimuli were adapted from those defined by Fitts *et al.* (1956) who called them "matrix" or "metric" figures. Such figures may be described by calling a 1/10 in. black square " ×," in which case examples of the set are as follows:—



Only 4 × 4 matrices were used in the experiment and these figures—of which there are 24—are built up on the basis of the combinations of 1, 2, 3 and 4. Thus, in matrix 1 (above), reading from left to right, the shape is characterized as: 1, 2, 3, 4; matrix 2 as: 1, 3, 4, 2; matrix 3 as: 1, 4, 2, 3; matrix 4 as: 1, 4, 3, 2, and so on. Each figure is unique in that none can be subtracted without remainder from any other (e.g. by superimposition) even though they derive from the same statistical family. Area is constant throughout, although permiter lengths are of three kinds: 12, 14 or 16 units. The advantage of using matrix figures is that they represent all the combinations in a certain formal system and consequently it is possible directly to compare performance on some of the figures with that on others. Further, it is thought to be advantageous to work with figures that differ minimally from one another, especially since these small differences are exactly specified. For the purpose of the experiment, the stimuli were adapted as follows. Firstly, to each shape was added its mirror reflection below the base line. Secondly, the shapes were randomly allocated to two groups. Group A remained vertically orientated and were thus called "vertical matrix figures," while group B were rotated through 90° clockwise and were called "horizontal matrix figures."

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* We announce with deep regret the author's death on 28th November, 1964. Acknowledgement is due to Professor R. L. Reid for his kindness in reading the proofs—*Editor.*

Method

Details of the general procedure have been given elsewhere (Butler, 1964). Unless otherwise stated, this procedure has been followed here. The apparatus consisted of the Staniland tachistoscope, a device having two wheels on which photographic slides can be mounted. By turning the wheels it was possible to select two of the slides (one from each wheel) for presentation. When the subject looked into the viewing box she saw a slide from the right-hand wheel exposed briefly. This was followed by an interval, after which the second slide—from the left-hand wheel—was exposed. Ten subjects (mean age 18 yrs. 6 mths.) were drawn from the same population as in the earlier study, but no subject took part in both.

There were 12 vertical matrix figures and 12 horizontal ones. Six of each kind were mounted on each wheel of the apparatus so that the right-hand wheel—for the first exposure—had 6 vertical matrix figures and 6 horizontal ones and the left-hand wheel—for the second exposure—had the same number of each. Whether any shape went to the left or right-hand wheel was determined on a random basis, but within the restraint just mentioned. The shapes were presented to subjects in pairs. These pairs were called "same" of "different." A same pair was defined as two horizontal matrix figures or two vertical ones, and a different pair as one horizontal and one vertical. A schedule of 60 trials was composed. In it there were 30 "same" trials and 30 "different" ones. The "same" trials were made up of 15 where both stimuli were horizontal matrices and 15 where both were vertical. The different trials had an equal number of occasions when the horizontal figure was presented first and when the vertical figure was presented first. For all trials the stimulus to be displayed was randomly selected from the available classes. Subjects were given no practice period but were allowed to examine cards on which examples of horizontal and vertical matrix figures were drawn. They were told that they were required to distinguish between different orientations, pairs of cards being ostensibly defined (by the experimenter) as "same" or "different", i.e. either two horizontal or two vertical figures or one of each. Further cards were shown and the subjects were asked to classify their patterns. When they achieved four errorless trials, the experiment continued. It is important to remember that there were no two stimuli that were objectively the same—the ensemble was composed of 24 unique items. Instructions served to direct the subject to respond on the basis of the orientation of the figures.

Results

Table I shows results that concern horizontal and vertical matrix figures and it can be seen that reaction times to pairs of horizontally orientated shapes are faster than reaction

TABLE I

MEAN RESPONSE TIMES (EXPERIMENT I)

Subjects	*"Same" pairs*		*"Different" pairs*
	Horizontal	*Vertical*	
1.	1·608	1·595	1·595
2.	1·222	1·297	1·299
3.	1·111	1·189	1·164
4.	0·817	1·156	0·806
5.	0·878	0·927	1·000
6.	1·113	1·208	1·035
7.	0·965	1·079	0·980
8.	0·987	1·198	1·047
9.	0·924	1·051	0·895
10.	1·423	1·471	1·466

The difference between horizontal and vertical pairs (mean response times) is significant at better than the 0·05 level (Wilcoxon matched pairs—2-tailed).

times to vertical pairs. (Wilcoxon matched pairs test—2-tailed.) This tallies with former results when this material has been used. (Fitts *et al.*, 1956.)

It has been noted that there were perimeter differences (doubled when the reflections were added); but there was a minimal difference between the *total* lengths of the perimeters of the horizontal and vertical figures, amounting to 3·7 per cent of the latter. Also, examination of the reaction times to pairs of shapes whose perimeters differed most and those to pairs of shapes whose perimeters did not differ indicated that discrepancy of perimeter length was not a contributory factor to the reaction times. This observation is, of course, limited to the experimental situation described here.

EXPERIMENT II

Introduction

Since minor variations in perimeter length did not affect response times in experiment I, it was asked: what would be the result of varying perimeter length by stages? As a further variation, instead of being symmetrical either from left-to-right or from top-to-bottom, shapes for experiment II were symmetrical both ways. A square of 10 × 10 units was taken as a standard figure and either its lateral extent or its vertical extent was modified by unit steps. Thus, two ensembles were generated (A and B). In each ensemble there were seven shapes, numbered from 1 to 7. The ensembles started with shape number 1, which was a square of 10 × 10 units, a unit being arbitrarily defined as 1/20 in. Shape 2 (ensemble A) differed from shape 1 in that its height was increased by one unit. The height of shape 3 was increased by a *further* unit, and that of shape 4 by yet one more unit. So the heights of shapes 1, 2, 3 and 4 were 10, 11, 12 and 13 units respectively. Shape 5 was generated by decreasing the height of the original square by one unit, shape 6 by decreasing it by two units and shape 7 by decreasing by three. Overall, then, ensemble A had seven members whose height varied by steps of one unit from seven units to thirteen units. All members had the same width. With ensemble B the converse applied: the height was held constant and the width of the figures was changed by one unit steps from a minimum of seven to a maximum of thirteen.

Method

A series of 84 trials was composed. Half of these trials were "same" ones and half "different." (The experimental procedure outlined earlier was used, except that, half way through the experiment, the first or brief exposure was increased from 0·2 sec. to 0·5 sec. This was done to reduce the number of errors being made.) Where the same trials were concerned, two identical shapes were displayed, one on each wheel of the apparatus. A different pair was made up of any of the combinations available from each ensemble: shape 1 was paired with 7, 4 with 5, and so on. In each ensemble there were 21 such combinations, and since each combination was presented twice, this made 42 trials. The other 42 trials were the result of presenting each shape and a copy of it six times each. Ten experimentally naive subjects (from the original source) took part in the work. Five of these subjects were first presented with ensemble A, five first presented with ensemble B. Then each group was given the other ensemble.

The amount of difference (in terms of increase or decrease of one or other extent) was not without variation. If shapes 1 and 4 of ensemble A were displayed, the difference was 3 units, but if shapes 1 and 2 were displayed (from the same ensemble), the difference was only 1 unit. Out of all the combinations of each ensemble there were the following differences, all in terms of the basic units:

12	pairs with a difference of	1 unit
10	do.	2 units
8	do.	3 do.
6	do.	4 do.
4	do.	5 do.
2	do.	6 do.

Before the experiment, subjects were shown cards bearing the ensemble they were to use and were told that: (i) an equal number of same and different pairs would be presented, (ii) by "same" was meant two copies of shape 1 or two of shape 4, etc., (iii) by "different" was meant any two shapes from the series, e.g. 1 and 7, 4 and 2, etc. Each subject had four preliminary trials, randomly selected from the experimental schedule, in order to ensure that the correct procedure was clearly understood.

Results

There were two measures of task difficulty: reaction times and errors. An error was registered when the subject classified the shapes as "same" and they were different or *vice versa*. Differences between pairs of shapes are called "shifts": shape 1 differs from shape 2 by one shift and thus between pairs of different shapes there are 1, 2, 3, 4, 5 or 6 such shifts.

Table II shows overall means for shifts. When the shift is small (shift 1) the reaction time is long (mean 1·322 sec.) and when the shift is large (shift 6) the reaction time is short (mean 1·092 sec.). Table III shows errors for shifts. Where the shift is a small one, there are large numbers of errors (45%); but where a shift of 6 is involved, the errors are few (12·5%). Both the error score and the reaction times therefore suggest that, as the shifts become larger, the task gets easier. Table IV details errors for same shapes. Comparison of these results with those in Table III reveals that there is a greater percentage of errors on same than on different shapes. Also, there were more errors associated with ensemble B than with ensemble A. Table V confirms this result by showing that reaction times to shapes in ensemble B were significantly slower than those to ensemble A (Mann-Whitney U Test—2 tailed).

TABLE II

Overall Means for Shifts

Shifts	1	2	3	4	5	6
Ensemble A	1·289	1·245	1·199	1·200	1·202	1·041
Ensemble B	1·355	1·265	1·251	1·307	1·194	1·144
Ensembles A and B ..	1·322	1·255	1·225	1·253	1·198	1·092

TABLE III

Total Errors for Shifts

Shifts	1		2		3		4		5		6	
	A	B	A	B	A	B	A	B	A	B	A	B
Ensemble	45	63	28	36	15	23	10	18	8	3	1	4
Per cent errors	45·0		32·0		23·7		23·3		13·8		12·5	

TABLE IV

Total Errors for "Same" Shapes

Shapes	7–7		6–6		5–5		1–1		2–2		3–3		4–4	
	A	B	A	B	A	B	A	B	A	B	A	B	A	B
Ensemble ..	23	26	20	22	20	20	19	14	22	23	20	28	17	27
Per cent of total exposures	81·7		70·0		66·7		55·0		75·0		80·0		73·3	

Discussion

The matrix figures used here lend themselves to an examination of Sutherland's suggested projectional analysis (Sutherland, 1960) which postulates that the octopus discriminates shapes on the basis of differences existing between the vertical and horizontal extents of the shapes concerned. The horizontal and vertical projections of the matrix

TABLE V

MEANS FOR "SAME" SHAPES

Shapes	7–7	6–6	5–5	1–1	2–2	3–3	4–4
Ensemble A ..	1·209	1·228	1·309	1·271	1·229	1·150	1·203
Ensemble B ..	1·253	1·413	1·439	1·265	1·308	1·351	1·261
Ensembles A and B	1·231	1·325	1·374	1·268	1·268	1·254	1·232

The difference in performance on Ensembles A and B is significant at better than the 0·02 level of confidence. (Mann-Whitney U Test.)

figures can be specified exactly because the latter are made up of squares of a constant size. The horizontal and vertical figures used here have been scrutinized from the point of view of quantifying their projections and seeking a characteristic difference between the groups, (horizontal *versus* vertical); but the evidence, although suggestive, is not conclusive enough to be detailed here.

Remarks of Fitts *et al.* are germane to the results of experiment I: "The finding regarding the superior performance with vertically orientated bilaterally symmetrical figures supports one of the incidental findings from an earlier study (Adams *et al.*, 1954) in this series, and might have been predicted in the light of observations from other types of investigations (Koffka, 1935; Wertheimer, 1945). Data from tracking studies (Fitts and Simon, 1952) also indicate that subjects show greater facility in responding concurrently to two stimuli that are symmetrical around a vertical axis (left-right separation) than around a horizontal axis" (*ibid.* p. 10). It is also worth noting that, with a number of species, up-down mirror image figures (such as Ts) are more readily differentiated than left-right ones. (Lashley, 1938; Harlow, 1945; Sutherland, 1960; Rudel and Teuber, 1963; Butler, 1964). For in all the cases cited the figures have been symmetrical either vertically or horizontally and those with vertical symmetry appear predominant, despite considerable variation of contour (cf. the results of experiment I with the results using T figures).

The fact that performance on vertically extended or reduced shapes in experiment II was better than on the horizontal ones is incidentally supported by much evidence from the work on the so-called horizontal-vertical illusion. (See, for example, Finger and Spelt, 1947). But again this result implies the pre-eminence of the vertical axis of symmetry even within shapes where the horizontal factor is present. Further work will be designed to examine this feature of two dimensional shapes more closely. The hallmark of such work will be the systematic and minimal modification of stimuli partly developed here.

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A NOTE ON FURTHER EXPERIMENTS ON THE EFFECTS OF MEPROBAMATE ON DISCRIMINATION PERFORMANCE IN THE MONKEY

BY

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A previous experiment (Gross and Weiskrantz, 1961) has shown that performance on a successive auditory discrimination task is impaired by injections of meprobamate. The present two brief experiments indicate that the drug also impairs simultaneous visual discrimination performance, although not to the same degree as found earlier for auditory discrimination. The original finding, therefore, cannot be attributed simply to unique features of the auditory discrimination situation, such as the "go—no-go" response contingency. Since neither overtraining nor drug habituation appears to be of great importance, it is suggested that the lesser effect of the drug in these experiments reflects the greater stability of visual than auditory habits in the monkey.

Earlier work has shown that meprobamate and reserpine can cause a severe deterioration in auditory discrimination performance of monkeys (Gross and Weiskrantz, 1961). This result was taken as supporting a hypothesis, growing out of still earlier research, that tranquillizers decrease the utilization of sensory information (Weiskrantz and Wilson, 1956; Weiskrantz, 1957).

The auditory task, as is almost always the case with monkeys as subjects, involved a "go—no-go" type of response contingency—i.e. the animal had to respond to the positive stimulus and had not to respond to the negative stimulus in order to achieve reward. It might be objected, therefore, that the deterioration in performance was associated not with discrimination as such but with the animals' willingness to perform at all. In fact, it was found that the animals tended to make most of their errors under the drugs by responding when they should not rather than not responding when they should.

The purpose of the present experiments, therefore, was to test the animals with a visual discrimination task in which both the positive and negative stimuli were presented simultaneously. An effect of the drug could not, in this situation, be characterized simply as altering the responsiveness of the animal. A further purpose in using visual stimuli was to test the generality of the earlier finding in a sense modality other than audition.

METHOD

Subjects

Two rhesus monkeys were used, one weighing 3 kgm., and the other 5·25 kgm. Both had been used in a variety of other experiments previously.

Apparatus

The experiments were carried out in a Wisconsin General Test Apparatus. The experimental room was darkened, the only illumination being a 60-watt frosted bulb in the top of the apparatus. Pairs of stimuli were presented simultaneously over wells located 12 in. apart, a peanut having been placed under one of the two stimuli.

EXPERIMENT I

Test Procedure

Visual stimuli. The stimuli were constructed of plastic tape mounted on painted plaques 4 in. × 4 in. and consisted of complex swastika variations shown in Figure 1.

Pre-drug training schedule. Fifty trials (at the rate of approximately three per min.) were given each day until the animal achieved at least 90 correct choices out of a possible

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* This work was carried out when Dr. Baltzer was on leave of absence from the Dept. of Pharmacology, Ciba, Basle, Switzerland.

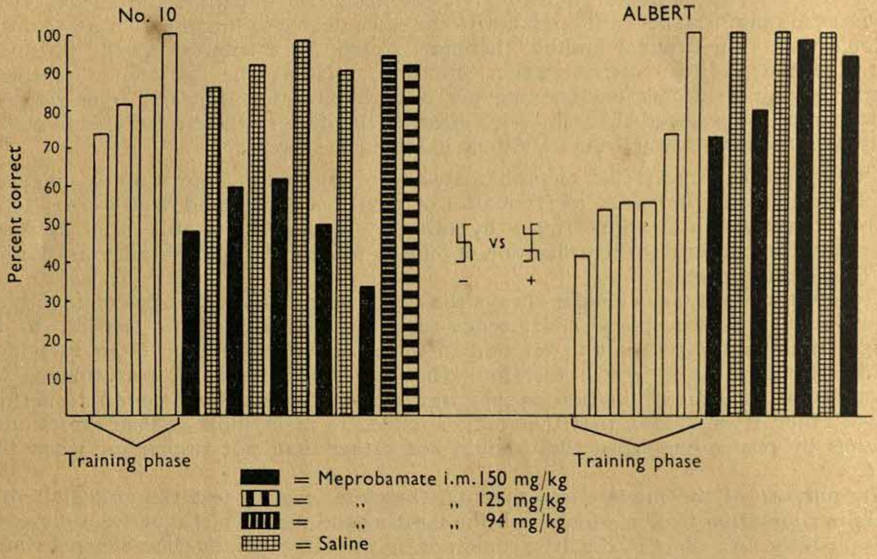
100. No correction trials were given. The position of the positive stimulus was varied from trial to trial according to a Gellerman random schedule.

Drug sessions. On the day following the achievement of criterion each animal was tested after an intramuscular injection of meprobamate, 150 mgm./kgm., in a vehicle of polyethylene glycol 400 (Wallace). Thereafter the animal was tested on alternative days receiving an injection of an equivalent volume of physiological saline or meprobamate. One of the animals was also given two lower doses of meprobamate (94 mgm./kgm. and 125 mgm./kgm.). There was always an interval of one hour between injection and testing. Each testing session consisted of 50 trials.

RESULTS

The results are depicted in Figure 1. Both animals acquired the discrimination in 200 to 300 trials. In one animal (No. 10) there was a marked and repeatable effect of meprobamate over all five testing sessions as compared with saline or no treatment. The other animal showed a moderate deterioration in performance for two testing sessions, but subsequently performed at well above 90 per cent correct. This, however, was still below the saline control level. The smaller doses of meprobamate were without effect.

FIGURE 1



DISCUSSION

It would appear that, while the effect was quite clear in the case of one animal, it was only marginal with the other. In addition, the fact that strong but smaller doses with the first animal were ineffective suggests that the conditions for producing the effect were rather close to threshold. This may have been due to habituation to the repeated doses of the drug (Oxbury, unpublished), to a discrimination task which was too simple intrinsically, or to the fact that the discrimination became overlearned with repeated testing. It is not possible to test decisively for these various possibilities without a much larger group of animals so that controls could be counterbalanced, but an attempt was made to determine whether any one of these possibilities was of outstanding importance in Experiment II.

EXPERIMENT II

Subjects and Apparatus

The same as in Experiment I.

Test Procedure

Visual Stimuli. A graded series of triangles was constructed from green plastic sheets on white painted metal plaques. Each pair of stimuli consisted of isosceles triangles, base horizontal, of the same height (3¼ in.) but of different base lengths.

Pre-Drug Training. Both animals were trained on an easy discrimination (base length 3 in. vs. 1·6 in.), longer base positive. Then more and more difficult sets were given in which the bases approached each other in length. The larger of the two bases was always positive. Testing in this phase was stopped when a particular set of triangles could just be discriminated at 90 per cent level (i.e. the next pair in the series consistently yielded performance levels under 90 per cent).

Drug Habituation Series. After reaching the criterion just described, each animal was given two sessions (three days apart), one with saline and one with meprobamate. In order to minimize overtraining effect, only 25 trials were administered in each session. Subsequently four additional meprobamate injections were given on alternate days, but no discrimination testing was carried out. Then a fifth additional injection was given, followed by testing. All doses were 150 mgm./kgm. and the interval between injection and testing was always one hour.

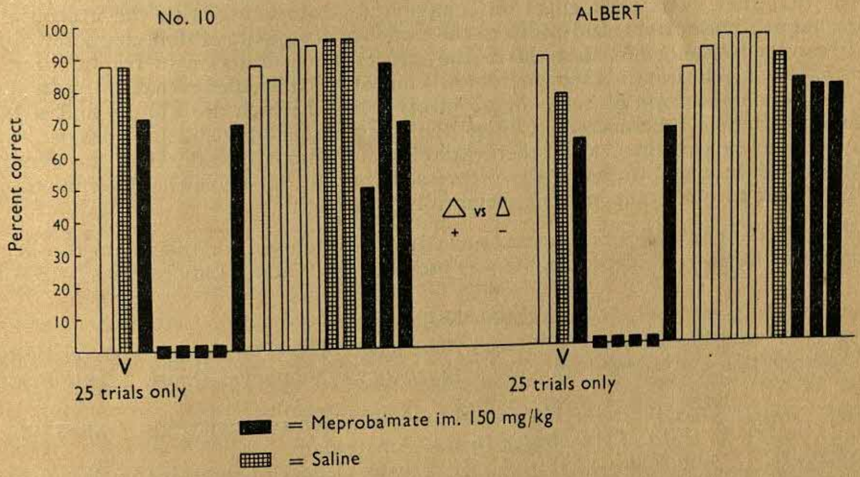
Overtraining Series. After the last session in the drug habituation series, both animals were given several additional discrimination sessions without drug treatment but terminating with saline sessions. These sessions were given daily. Finally three drug sessions, on alternate days, were given with discrimination testing. The size of the stimuli throughout this series was the same as in the drug habituation series.

RESULTS

Drug Habituation

The levels of performance under the drug before and after the habituation series were very close (Figure 2). For one animal post-habituation level was slightly lower than the pre-habituation level, for the other slightly higher. It is appreciated that strictly speaking a control comparison is necessary with a group which had received no drug injections during the habituation series, in order to test for the effect of passage of time on retention of the discrimination habit. But it is very unlikely that retention would suffer at all over the ten-day habituation period.

FIGURE 2



Overtraining Series

In the case of one animal (No. 10) the post-overtraining drug effect was approximately the same as before the overtraining series, for the other there was an improvement of 10 per cent. In the case of both animals, however, there was still an appreciable drug effect, although Albert performed at about 80 per cent correct.

DISCUSSION

It is evident that meprobamate will cause a deterioration in performance of visual discrimination, although possibly not to the same degree as was found earlier for auditory discrimination. The effect, therefore, cannot be attributed simply to unique features of the auditory discrimination situation employed, such as the successive presentation of the stimuli, the "go—no-go" response contingency, or, of course, to the auditory modality itself.

Since neither the drug habituation series or the overtraining series produced marked improvements in performance under the drug, the most likely explanation of the slight effect seen in one animal in Experiment I was the relatively high discriminability between the stimuli, combined, perhaps, with some contribution from overtraining. Auditory discriminations appear to be rather more difficult and unstable for monkeys than visual discriminations and perhaps the drug effects may therefore be more sensitively measured. Even so it should be noted that there was considerable variation in mean performance of monkeys in the auditory study under the drug, ranging from 52 per cent to 77 per cent (chance level was 50 per cent), and no doubt there are differences in susceptibility to the drug itself. The fact that a reduction in dosage of 17 per cent in Monkey No. 10 abolished its effect on visual discrimination (Experiment I) means that differences in susceptibility may be quite critical when the effect is near threshold.

The fact that moderately high doses appear to be necessary to produce the effect (even though their only effect on gross behaviour was occasional slight inco-ordination) raises the point of the general characterization of the drug action on behaviour. It seems plausible, because of the high doses, that the drug shares many of the features of central nervous system depressants not normally classified as "tranquillizers," and in a subsequent paper we hope to show that the effects of meprobamate and sodium pentobarbitol are indistinguishable in a particular behavioural situation.

The fact that not all of the properties of the drug are unique to its class of drugs does not mean that the unique features are necessarily the ones which determine its clinical usefulness. As noted above, we have argued that a general characterization of tranquillizers is that they exert their effect on behaviour by interfering with the utilization of sensory input. The current study adds to the evidence in support of that characterization. But it cannot be concluded that this is the critical mode of action of tranquillizers, or even that it is relevant to their usefulness clinically. It is likely that all such drugs produce a wide spectrum of behavioural effects some of which are unique and some of which are shared by other classes of drugs, the spectrum possibly shifting considerably as a function of dosage, time, and other variables. It cannot be said that the empirical description of these spectra has really progressed very far, let alone the determination of which of their specific components are clinically important.

We wish to thank Miss S. Hopkins and Mr. R. Hutchison for their help throughout the course of the study. Meprobamate was provided by Wallace Laboratories (U.S.A.).

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THE AFTER-EFFECT OF SEEN MOTION: THE ROLE OF RETINAL STIMULATION AND OF EYE MOVEMENTS

BY

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An experiment is described in which movement after-effects are noted, following presentation of moving stripes under various conditions of eye movement. After-effects only occur when the retinal image moves systematically across the retina, though movement may be observed when this is not the case. The after-effects are due to specifically retinal stimulation, not to perception of movement *per se*.

A moving object can be detected in one of two ways. The object can be tracked by the eyes, keeping its image stationary on the retina, or the eyes can remain fixed on some stationary point, so that the image of the object moves across the retina. These two methods involve what have been termed the "eye/head" and the "image/retina" systems (Gregory, 1958). These two kinds of information are combined in the nervous system to give our subjective perceptions of movement: Teuber (1960) discusses how this may be done.

The well-known after-effect of seen motion represents some modification or adaptation of the movement detection system. In this study the "eye/head" and the "image/retina" systems were stimulated separately and together to establish which system mediates the after-effect.

Six subjects, aged 22–27, inspected a field of horizontally moving stripes for 45 sec. After this stimulation, they immediately looked at a randomly patterned surface (a photograph of sandpaper) and reported the presence and direction of any after-effect. Five conditions were used, and the results are shown diagrammatically in Figure 1.

The stimulus field was composed of 3 mm. black and white stripes, moving to the right at 10 mm./sec. surface speed (¾° visual angle per sec.). A fixation spot was visually superimposed via a half silvered mirror: it could be stationary (a bulb filament) or moving (an oscilloscope spot).

The Conditions and results were:

Condition A: Stationary fixation spot, stripes moving to the right. The images of the stripes thus move across the retina of the stationary eye.

Result: After-effect to the *left*—the opposite direction to the stimulus stripe motion.

Condition B: Subject tracked stripes with the eyes, keeping the image, ideally, stationary upon the retina. (This was done by making the fixation spot move along with the stripes.) Here, the eyes move but the retinal image does not shift across the retina.

Result: No after-effect.

Condition C: The stripes were stationary, while the eyes tracked smoothly across them, following a fixation point moving to the right at 37·5 mm./sec. Here the eyes move in one direction while the retinal images move in the opposite direction; thus, the eye/head and image/retina systems receive opposite signals.

Result: During the stimulation period the stripes appeared to "sail" to the left; thus it appears that the image/retina signals dominate during the stimulation. The after-effect was to the *right*—appropriate to retinal stimulation.

Condition D: The stripes moved to the right at 10 mm./sec. surface speed, and the fixation spot moved also to the right, but at 37·5 mm./sec., overtaking the stripes. The eyes, tracking the spot, moved to the right but the retinal stimulation was the same as though the stripes were moving to the left. In other words: the eyes move in one direction, with perceived movement in the same direction though the retinal images move in the opposite direction.

Result: After-effect to the *right*. This is appropriate to the retinal stimulation, and not to the perceived direction of the movement.

FIGURE 1

Condition	A	B	C	D (E = C + D)
Perceived movements				
Stripes	→	→	×	→
Fixation point	×	→	⟶	⟶
Stimulation				
Retinal image	→	×	⟵	←
Eye movements	×	→	⟶	⟶
Physical movement	→	→	←--	→
Direction of after-effect	←	×	→	→

For simplicity, the inversion of the retinal image has been ignored. Thus an arrow to the right in the "retinal image" row indicates retinal stimulation appropriate to an object moving to the right past a stationary eye. This does not affect the results.

Note that the after-effect is in the opposite direction to the retinal stimulation in each case. This is not true of any other stimulus parameter. This indicates that the retinal stimulation alone is responsible for the after-effect.

Condition E: Here Conditions C and D were combined. The upper third of the striped field was stationary (as in C) while the lower two-thirds moved to the right (as in D). The fixation point, located in the lower two-thirds, moved at 37·5 mm./sec.

Result: An after-effect in the upper part of the field (Condition C) and *no* after-effect—or a very minimal one—in the lower part (Condition D). So the after-effect is much stronger when the stripes are stationary than when they are moving slowly in the same direction as the fixation spot.

It follows from these results that the motion after-effect is determined by the *retinal* stimulation, and is independent of eye movements, or of the subjectively perceived motion which results from a combination of eye/head and image/retina information.

This work was carried out while S.M.A. was receiving a DSIR research studentship.

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PREDICTABILITY AND IMMEDIATE MEMORY FOR CONSONANT SEQUENCES

BY

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Two measures which have been shown to predict the ease of learning trigrams, namely log letter frequency and sequential predictability, were applied to data from an experiment on short term memory. This involved the immediate recall of 120 six-letter consonant sequences which were presented visually one letter at a time. A significant correlation was found between the probability that a given sequence would be recalled correctly and both its mean log letter frequency ($r = 0 \cdot 308$, $p < 0 \cdot 001$), and its mean predictability ($r = 0 \cdot 393$, $p < 0 \cdot 001$). Partial correlation showed only a marginally significant effect of log letter frequency when predictability was partialled out ($r = 0 \cdot 161$, $0 \cdot 05 < p < 0 \cdot 1$). With log letter frequency partialled out, however, a reliable correlation between predictability and recall score remained ($r = 0 \cdot 300$, $p < 0 \cdot 001$).

INTRODUCTION

Predictability (Baddeley, 1961, 1964) is a term used to define a relationship between sequences of letters of the alphabet. When predictability is high the digram structure of the sequence approximates that of ordinary language. It is an objective index, in that the measure is based on a large sample of real language, and it can be applied to sequences of any length.

The predictability of a letter sequence is computed as follows. A statistical "subject" is assumed who attempts to guess each of a sequence of letters, given the previous letter, on the basis of the digram structure of printed English (Baddeley, Conrad and Thomson, 1960). The mean number of "guesses" required per letter is the measure of predictability and indicates how closely the digram structure of any sequence approximates to that of English. The closer a sequence approximates to English, the fewer the "guesses," the lower the score and the easier it will be to learn.

It is common experience in experiments on immediate memory for digit or letter sequences, that the ease with which different sequences are memorized varies greatly, even when they are constructed to identical rules. Even when commonsense inspection suggests no reason why one sequence should be easier than another, it may nevertheless be recalled by five or six times as many subjects.

There has in recent years been a revival of interest in the question of what makes verbal material easy or difficult to remember. Associative value (Archer, 1960, Noble, 1952, 1961), "pronunciability" (Underwood & Schulz, 1960), letter frequency (Di Mascio, 1959), and letter sequence predictability (Baddeley, 1961, 1963) have all been claimed as crucial determinants of ease of learning. While most of these studies have been concerned with long-term memory, there is also evidence for similar relationships in immediate memory. Miller (1958) and Staniland (1960) have shown that subjects can improve their performance by making use of constraints introduced into the recall material. Peterson, Peterson and Miller (1961) found that short term retention of C.V.C. trigrams was related to associative value, with words easiest, high associative value nonsense syllables of intermediate difficulty and low associative value syllables hardest to recall. They concluded, however, that these differences were due to letter sequence habits rather than to mediation from meaningful associations. A similar interpretation was suggested by Baddeley (1964) who found a highly significant correlation (rho = 0·80) between short term memory for 8-letter sequences and their predictability. The material used was taken from Miller, Bruner and Postman (1954) and ranged from real words, through various approximations to English, to random sequences, each being typed on a card and presented as a single item. The present study applies both this measure, and Di Mascio's measure of letter frequency, to material used in a quite separate study (Conrad, 1964) in which only consonants were used, and where the letters were presented sequentially. Conrad's material varied in both letter frequency and predictability and showed considerable differences in ease of recall. As this type of material is frequently used in studies of immediate memory, any success in predicting item difficulty is likely to be of value in the future design of such experiments.

METHOD

Material

The details of the method used to obtain recall scores are more fully described by Conrad (1964). Briefly, a set of 120 six-letter consonant sequences was prepared using a vocabulary of 10 consonants (B,C,P,T,V,F,M,N,S,X). Letter order was basically random with the constraint that each letter and each digram occurred approximately equally often in each serial position, and no letter occurred more than once in any sequence. Examples of the sequences used would be MVSCTN (mean predictability 10·8 "guesses" per digram—"high" predcitability), and FXSBPV (mean predictability 16·6—"low" predictability). The sequences were photographed one letter per frame on 16 mm. film, each sequence being preceded by three "ready" frames carrying a spot.

Procedure

Sequences were presented visually using a frame by frame 16 mm. film projector. Each letter was visible for 0·75 sec. After the sixth letter the projector was stopped until all subjects were ready for the next sequence.

The subjects were 387 trainee telephonists. They were tested in groups of about 10, each of which did 40 sequences in a design which ensured that all 120 letter sequences were used approximately equally often. The 10-letter vocabulary was displayed on a blackboard throughout to avoid extra-vocabulary intrusions, and subjects wrote their response on a printed answer sheet.

RESULTS

Recall Score

The frequency with which each sequence was reproduced completely accurately was noted and transformed to give the probability of correct reproduction (pC). Considerable inter-item differences occurred, with pC ranging from 0·13 to 0·72 with a mean of 0·31 (S.E. = 0·10).

Letter Frequency Score

This was computed as described by Di Mascio (1959) by totalling the log frequency of occurrence in English of the six individual letters in each sequence and taking the average, giving a frequency score for each of the 120 items. Letter frequencies, were, however, based on the Baddeley, Conrad and Thomson (1960) count of letter frequency in a 13,000-word sample of printed English, rather than on Pratt's count, which is based on 1,000 words from an unspecified source (Pratt, 1939). The material had a mean log frequency of occurrence of 3·249 per letter (S.E. = 0·083), a value about half way between that expected by chance (3·118) and that of English prose. (The mean log letter frequency score for the present sentence for example, is 3·364.) When log frequency was correlated with pC across the 120 sequences using a product moment correlation, it yielded an r of 0·308 ($t = 3{\cdot}67$, $p < 0{\cdot}001$). This and all subsequent significance levels are based on two-tail tests.

Predictability

This was calculated as described previously. Sequences had a mean predictability score of 14·55 guesses per digram (S.E. = 1·48), which is less predictable than one would expect a random selection from all 26 letters to be (mean expected predictability 13·5). Product moment correlation between predictability and pC gave a value of 0·393 ($t = 4{\cdot}14$, $p < 0{\cdot}001$).

Relationship between Predictability and Letter Frequency

It seemed probable that these measures would be quite highly correlated, as predictable sequences are likely to comprise frequent letters. There proved to be correlation of 0·448 between the two variables. On the basis of this, two partial correlations were computed (Kendall, 1952). These showed a correlation between predictability and pC of 0·300 ($z = 3{\cdot}34$, $p < 0{\cdot}001$) when letter frequency was partialled out, and a correlation of 0·161 ($z = 1{\cdot}75$, $0{\cdot}05 < p < 0{\cdot}1$) between log letter frequency and pC, when predictability was partialled out.

DISCUSSION

It seems clear that there is a small but consistent correlation between predictability and ease of recall. There may also be a correlation between pC and log letter frequency, when predictability is partialled out though this is of marginal statistical significance. The question arises as to why the correlation between *pC* and predcitability should be so much lower than the correlation of 0·80 obtained by Baddeley (1964). There are a number of fairly obvious reasons. Firstly, the range of predictability is much narrower in the present study (10·2—19·1 as compared with 2·2—18·2 guesses per digram). Secondly, the letters were presented sequentially rather than simultaneously, and this, combined with the complete absence of vowels makes the material so different from normal language that it is perhaps surprising that subjects' language habits should have been of any assistance at all in coding and remembering the material. Finally, Conrad's study used an all-or-none recall score, whereas Baddeley scored number of letters correct per sequence, which might be a more sensitive index of sequence difficulty.

It is possible though that some differences in meaningfulness do still occur. For example, the easiest sequence by far was STMPBX, which is not only the most predictable sequence, but is also relatively meaningful both because it suggests the words "stamp box" and because the initials P.B.X. (Private Branch Exchange) were very familiar to this sample of subjects. This, however, is very much an exception, and it seems very unlikely, for example, that BFNCST (predictability 10·57), the next item in order of difficulty, is very much more meaningful or pronounceable than FMTPNV (predictability 18·14), the hardest sequence; especially if we remember that subjects read only one letter at a time.

However, even if the concepts of meaningfulness and pronouncibility *can* justifiably be applied to long sequences devoid of vowels, their estimation is laborious, requiring as it does a major scaling experiment whenever a fresh sample of material or a new source of subjects is used. On the other hand, the predictability of a fresh sequence can be computed in a matter of seconds and is applicable to any English-speaking subject.

We do not therefore claim that predictability is the only source of relevant variation between consonant sequences. But it is clear that predictability is an easily controlled factor which does help to determine whether or not a sequence will be easy to recall. The implications of this for experimental design in the field of immediate memory will be obvious.

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THE EFFECT OF OVERLEARNING ON TRANSFER OF TRAINING

BY

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The effect of overlearning on transfer of training on the A-B:A-B_r* paradigm was studied in paired-associate learning with meaningful material (adjective pairs). One group of subjects was trained to criterion on list A-B, and two additional groups were given 100 per cent and 200 per cent overlearning on list A-B. Rate of learning list A-B_r was found to be directly related to amount of overlearning. Negative transfer on list A-B_r was found for errors with the criterion group, while positive transfer was found for the 100 per cent and 200 per cent overlearning groups. The results were consistent with previous paired-associate experiments, and with results of some maze, reversal learning experiments with infrahuman subjects.

INTRODUCTION

A number of experiments on the effect of overlearning on transfer of training with human subjects learning paired-associate material (e.g. Mandler, 1954; Siipola and Israel, 1933) and with infrahuman subjects learning reversal responses in mazes (e.g. Capaldi and Stevenson, 1957; Reid, 1953) have demonstrated that the amount of positive transfer is directly related to the degree of overlearning. (Henceforth, this phenomenon will be referred to as the "over-learning effect.") These two classes of experiments are similar in that (*a*) both entail two successive learning tasks which differ in some important respect, and (*b*) both are concerned with the effect of learning the first task on learning the second task. Although Mandler (1962) observed these similarities, he viewed the maze reversal learning experiments as analogous to the A-B:A-C transfer paradigm, and compared them to paired-associate transfer experiments in this paradigm. However, the maze reversal learning problem is actually in the A-B:A-B_r paradigm because a previously incorrect response option becomes the correct response upon reversal learning. In the A-B:A-C paradigm, a new set of responses are presented in the second phase which were not response options in the first phase. Thus, the most meaningful comparisons of infrahuman reversal learning experiments are with human paired-associate transfer experiments in the A-B:A-B_r paradigm.

Several experimenters (Mandler, 1954; Mandler and Heinemann, 1956; Siipola and Israel, 1933), all of whom used response members of low meaningfulness (e.g. telegraphic codes, nonsense syllables), have reported the overlearning effect with the A-B:A-B_r transfer paradigm for human subjects. Considering only the subjects trained to a criterion of learning, the usual marked negative transfer to the second (i.e. reversal) learning problem found in maze reversal learning experiments (Mandler, 1962) was not evident in these paired-associate experiments. In other paired-associate experiments (Porter and Duncan, 1953; Twedt and Underwood, 1959) using meaningful material, significant negative transfer in the A-B:A-B_r paradigm has been found when the A-B list was learned to the criterion of one errorless trial. *For groups trained to criterion*, negative transfer in the A-B:A-B_r paradigm apparently increases with meaningfulness of material, and the amount of negative transfer found with meaningful material resembles more closely the amount of negative transfer found in infrahuman reversal learning. Because no experimental evidence is available on the overlearning effect with meaningful material, no comparisons for groups trained beyond criterion can be made. The present experiment was undertaken to examine the effect of overlearning on transfer of training in the A-B:A-B_r

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* The notation "A-B:A-B_r" will be used for the transfer paradigm in which the same stimuli and responses are used in both the original acquisition phase of paired associate learning and in the subsequent transfer of training phase. In the transfer phase, however, the responses (i.e. B) are rearranged so that they are paired with different stimuli (i.e. A). The notation "A-B:A-C" will be used for the transfer paradigm in which a new set of responses (i.e. C) are paired with the old set of stimuli in the transfer of training phase.

paradigm with meaningful material (adjective pairs) so that further comparisons could be made with paired-associate experiments using response members of low meaningfulness and with reversal learning experiments.

METHOD

Subjects. The subjects were 30 students (10 men, 20 women) randomly selected from a population of approximately 300 students enrolled in a course in introductory psychology. They had no previous experience as subjects in a verbal learning experiment.

Lists and Apparatus. Two lists were constructed for the A-B:A-B_r transfer paradigm. The A-B list was composed of seven adjective-adjective pairs randomly selected from Schultz and Rundquist's (1960) list. The A-B_r list was constructed by randomly rearranging the response members, with the limitation that they could not be paired with their former stimulus member. Ten different orders of both lists were prepared as a control for serial effects. The lists were presented on a Gerbrands memory drum modified so that longer tapes than standard could be used.

Procedure. The 30 subjects were randomly assigned to three groups of 10 subjects each. Group I was trained to the criterion of one errorless trial; Group II was given 100 per cent overlearning; Group III was given 200 per cent overlearning. Each subject in Group II was given twice as many training trials as he required to reach the criterion of one errorless trial, while the number of trials required to reach the criterion of learning for each subject in Group III was tripled.

Experimental sessions were conducted individually by one experimenter. Standardized instructions which described the experimental situation and the task were read to the subjects. The word pairs were presented at a 2:2 second rate with a four second intertrial interval. Upon completion of training with the A-B list and a subsequent five minute rest interval, training to the criterion of one errorless trial on the A-B_r list commenced.

RESULTS AND DISCUSSION

The means and standard deviations of error scores and the number of trials to reach the criterion of one errorless trial on both lists are presented by Group in Table I. The results of the analyses of variance of these means are reported in Table II. The significant overlearning X list intereactions indicate that overlearning list A-B facilitated learning list A-B_r.

TABLE I

MEANS AND STANDARD DEVIATIONS OF TRIAL AND ERROR SCORES

Group	*Trials*				*Errors*			
	A-B		*A-B_r*		*A-B*		*A-B_r*	
	Mean	*s*	*Mean*	*s*	*Mean*	*s*	*Mean*	*s*
I	8·50	2·64	8·50	2·80	27·50	6·67	32·30	12·17
II	9·20	1·87	6·50	1·18	26·70	3·83	20·10	2·19
III	8·70	2·11	5·60	1·43	25·70	4·85	17·80	2·49

The progressive significant decrease in both errors and trials to criterion as a function of overlearning indicates that the overlearning effect also occurs in paired-associate learning with meaningful material. The usual negative transfer for subjects trained only to criterion was found for errors, but not for trials to criterion. Although Porter and Duncan (1953) obtained negative transfer for both dependent variables, they found considerably more negative transfer for errors than for trials to criterion. Thus, the results approximate those of other-comparable paired-associate experiments using meaningful material, and, in addition, show that transfer becomes increasingly positive with overlearning. This functional relationship is also quite similar to that found with infrahuman subjects in *some* reversal learning experiments, and suggests that the similarity between them and paired-associate transfer experiments may extend to the learning process as well as to the

TABLE II

Analyses of Variance for Trial and Error Scores

Source	*d.f.*	*Trials*		*Errors*	
		Mean Square	*F*	*Mean Square*	*F*
Overlearning (O) ..	2	9·12	1·63	371·32	12·77**
Between *S*s error.. ..	27	5·60	—	29·07	—
List (L)	1	56·06	17·91**	156·81	3·04
O X L	2	14·22	4·54*	244·12	4·73*
Within *S*s error	27	3·13	—	51·57	—

* Significant at 0·05 level.
** Significant at 0·01 level.

similarities in experimental procedure noted at the beginning of the Introduction. However, since the overlearning effect has not been found in about 50 per cent of the infrahuman reversal experiments, there are obviously important differences too.

Considering the failures to find the overlearning effect in infrahuman species (e.g. Boe, 1964; Erlebacher, 1963; Hill, Spear, & Clayton, 1962; Warren, 1960), the consistent demonstration of this phenomenon in human paired-associate learning is impressive. It has been demonstrated for both verbal and manual responses, and for responses of either high or low meaningfulness. In view of this reliability, it would be interesting to try using human paired-associate transfer experiments for evaluating the several hypotheses (Capaldi and Stevenson, 1957; D'Amato and Jagoda, 1961; Reid, 1953) that have been offered to account for this effect. It seems to be sufficiently unreliable in infrahuman species that research should first be devoted to ascertaining the parameters relevant to its demonstration.

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BOOK REVIEWS

Annual Review of Psychology. Vol. 15. 1964. Edited by Paul R. Farnsworth, Olga McNemar and Quinn McNemar. Palo Alto, California: Annual Reviews Inc. Pp. vii + 629. $9.00.

The present reviewer must confess to his shame that he has not read one of these *Annual Reviews* right through since Volume I (1950). The main changes since then appear to be (1) the size is doubled; (2) some non-Americans contribute; (3) the existence of psychology in the U.S.S.R. is acknowledged. Otherwise the mixture is much as originally prescribed.

In this volume are seventeen review articles, nicely divided between "fundamental" and "applied" topics. The first group comprise R. H. Day on perception; Judith W. Onley on visual sensitivity; D. N. Elliott on audition; R. W. Russell on psychopharmacology; D. E. Berlyne on learning; W. A. Mason and A. J. Riopelle on comparative psychology (specifically primates); Jack A. Adams on motor skills; Arnold Binder on statistical theory; and Eleanor E. Maccoby on developmental psychology. All these articles seem to an ageing reviewer to be up to date and competent within their respective conventions, and in several cases to show unexpected breadth in their coverage. Even if nothing startling emerges, the overall picture is distinctly encouraging.

Although it is somewhat invidious to distinguish between individual chapters, it is inevitable that some will stand out in the judgement of a non-expert reader. This reviewer particularly enjoyed Dr. Onley's article on vision, which is bright, informative and unusually readable. Dr. Roger Russell, as is his wont, tempers zest with caution in his review of psychopharmacology, but it is a pity he is so fond of abstract nouns. Dr. Adams has some nice things to say about British work on skill, and is pleasingly uninhibited in some of his judgements. (Incidentally, a little more plain speaking would not come amiss in these volumes generally.)

The second group of topics comprise J. Smedslund on educational psychology; J. E. Milholland on assessment techniques; K. M. Colby on psychotherapy; H. J. Leavitt and B. M. Bass on "organizational psychology" (which seems to mean group research and the study of decision processes); S. B. Sells on personnel management; I. D. Steiner on group dynamics; P. London and D. Rosenham on personality. Having read practically nothing on these topics for some years, the present reviewer can offer no comment. But Smedslund, of Oslo, seems something of a discovery. He is equally at home in the American and the European traditions of educational research and arbitrates nicely between them.

As has been said, Soviet psychology is by no means unrepresented. Berlyne, Mason and Riopelle, and Smedslund, all refer on occasion to recent Soviet work. This is as it should be. More doubtful, however, was the wisdom of including yet another mammoth review (by J. Brožek) of recent developments in Soviet psychology. Though learned in its way, this article is almost incomprehensible to those without special knowledge of the Soviet psychological scene. It might be a good idea for a later volume to get someone like Alexander Luria (who among his other talents writes excellent English) to prepare a straightforward account of what experimental psychologists in the U.S.S.R. actually do.

In the Cumulative Index of chapter titles for Vols. 11–15 appended to this volume, social psychology and its various subdivisions have been inexplicably omitted.

It is sometimes said that there is a tradition in this *Journal* of being beastly to the *Annual Review*, which undoubtedly reflects much that is most dispiriting in contemporary psychology. Yet one must feel real sympathy for the contributors in their unenviable task. This year, at least, they succeed surprisingly well in gathering together recent psychological work and ideas within the somewhat arbitrary arrangement of topics imposed by editorial convention. If the outcome has shortcomings, they are for the most part shortcomings not of the authors but of the subject itself in its current manifestations. Are we not trying to do much too much, much too quickly? Might it not be more to the point to limit these reviews to topics in which definite and agreed advances are being made, no matter on how narrow a front? If psychology goes on as it is it will soon cease to be even the hope of a science; it will become the graveyard of a profession.

O. L. ZANGWILL.

Bargaining Behaviour. By Lawrence E. Fouraker and Sidney Siegel. New York and London: McGraw Hill. 1963. Pp. ix + 309. 69s. 6d.

This book represents a pioneering attempt, by a psychologist and an economist working in collaboration, to apply experimental techniques developed in the study of psychology to some of the most difficult of economic problems. Economics has seldom, until now, used experiments, because there are obvious objections to conducting experiments on the economy itself and because it is difficult to produce in the laboratory conditions sufficiently analogous to those of the economic world. Perhaps because of this, economists, where they have done empirical work, have attempted to make inferences from the observed behaviour of the economy; and more commonly have eschewed empirical work altogether in favour of purely deductive analysis of what behaviour would be "rational." Such deductive analysis can be illuminating in relatively simple situations where the concept of "rational behaviour" is unambiguous; but bargaining problems are of such essential complexity that economic theorists can offer only a number of mathematical models, each based on a different, more or less plausible (but untested) hypothesis.

Fouraker and Siegel consider a number of bargaining problems differing in their detailed structure, and several theories relating to each; but, in each case, they are primarily concerned to contrast representatives of two groups of theories. Those in one group, postulating highly sophisticated behaviour, predict that the bargain struck will be "Pareto optimal"—that is, such that the benefit to any participant can be increased only by reducing that to one of the others. Those in the second group postulate that each participant bases his bargaining on some rather naive assumption about the behaviour of the others, and predict outcomes such that, by collaboration, the benefit to each participant could be increased. At first sight, the most striking of the Fouraker and Siegel results is that most of their experiments support the naive rather than the sophisticated theories; but the significance of this is considerably reduced by a number of differences between the bargaining situations of the experiments and those of the real world.

The first (probably relatively unimportant) difference is the almost inevitable one, that the laboratory experiments concern trivial sums of money whereas, in the real world, very large sums are often at stake. More importantly, while communication in the real world is effectively unlimited, in the experiments the participants were allowed to communicate only by quoting a single number (a price or quantity) and since only short sequences of such bids were permitted, the amount of communication was severely limited. Fouraker and Siegel stress that the provision of information about the benefit to others of each bargain increases the frequency of sophisticated behaviour, which supports the view that an important influence on their results is the difficulty of communicating adequately.

Again, the subjects were undergraduates (who were presumably relatively inexperienced in bargaining) and each experiment was conducted with a fresh group who were given little if any chance to practice; it is more surprising that in some experiments the participants were able to learn quite sophisticated bargaining techniques than that in others they failed to do so. In contrast, those frequently engaged in real-world bargaining have unlimited opportunities, and strong incentives, to develop highly sophisticated techniques. In fact, the authors report a similar experiment, by Siegel and Harnett, with business men as subjects, which suggests that business men learn the sophisticated techniques faster than undergraduates.

Thus the results are consistent with—although the experiments were not well designed to test—the proposition that, if permitted to learn and to communicate, subjects will tend to reach Pareto optimal bargains. Conversely, it seems well established that sufficiently restrictive conditions can prevent this from happening and that then some of the more naive economic theories may apply. Unfortunately, no light is thrown on what many economists would regard as the key question—if conditions permit a Pareto optimal outcome, what determines *which* Pareto optimal bargain will actually be reached?

Lack of space prevents us from considering here many other interesting results. For instance, the actual sequences of bids, which are published in full in the appendices, seem to merit much more analysis than they have received; although one wonders whether the trichotomy of "maximizing," "cooperative" and "rivalistic" is really helpful in analysing the complex purposes of information and misinformation, of threatening, bluffing, coaxing and capitulating, that must underlie these sequences.

If this review has been largely concerned with criticizing the design and interpretation of the experiments, this should not be allowed to prevent recognition of the vast amount of work reported in the book, the scrupulously detailed way in which it is reported and the pioneering nature of the whole enterprise. Indeed, Siegel's death represents a tragic loss to what, one hopes, may soon become the flourishing sub-discipline of experimental economics.

M. J. Farrell.

The Physiological Basis of Mental Activity. Editied by R. Hernández-Peón. Supplement, Number 24 to *Electroencephalography and Clinical Neurophysiology.* London: Elsevier. 1963. Pp. vi + 282. 90s.

This book contains the Proceedings of a Symposium held in Mexico City (but excludes any discussion that may have occurred of the papers). One of the objectives of this Symposium was to help to construct a ". . . bridge to fill the gap notoriously existing between the realms of the brain and mind . . .". To achieve this objective, construction work was divided among three groups whose contributions were classified under the headings Central Sensory Mechanisms (8 papers), Sleep Mechanisms (6 papers) and a heterogeneous group collected together under the heading "Learning, Motivation, Communication, Maturation and Social Behaviour" (5 papers).

The standard of the contributions ranges from quite good to appallingly bad. The paper by Brooks entitled "Variability and Redundancy in the Cerebral Cortex" is certainly among the best and the one by Mancia ("Specific and Unspecific Influences on the Olfactory Bulb") is a valuable paper, though it is marred by the unqualified assertion that topical application of drugs (to the cerebral cortex) cannot elicit antidromic stimulation of axons.

A number of papers are concerned with sensory evoked potentials in animals and man. Two of these contributions describe the morphology of these waveforms in great detail, but are not particularly relevant to the topic of the Symposium, since their treatment of these waveforms is largely descriptive. A paper by Creutzfeldt and Meisch attempts to relate cortical slow wave activity to impulse activity and they present some beautiful records, though one would have liked to see a more quantitative treatment of their material.

Some of the papers on sleep are of interest and are useful summaries of work up to 1961.

The great weakness of most papers lies in lack of quantitative data. Though it is sometimes justifiable to use words like "some," "a few," "representative records" and so on, it is nice, occasionally, to see rather more precise statements, so that the reader can assess for himself the weight of the evidence on which the author's conclusions are based. This criticism is especially true for data on slow wave activity (electroencephalogram, evoked potentials) and often true, though to a lesser extent, of impulse activity. Some of the parameters of these bioelectrical phenomena show considerable "spontaneous" change and it is often quite difficult to decide, for a given set of changes, whether they occurred spontaneously or as a result of experimental manipulation. Only rarely are adequate steps taken to apply the controls necessary for this distinction to be made; so one is left wondering.

In Symposia of this kind, authors are frequently allowed some poetic licence, but one wonders if this can justifiably be extended so far as to allow the assertion that an unanaesthetised animal, held in such a position as to prevent head movements, was "comfortable" (p. 33), or to permit pseudocholinesterase to be referred to as a transmitter substance (p. 239).

G. HORN.

Substances Producing Pain and Itch. By C. A. Keele and Desirée Armstrong. London: Arnold. Monographs of the Physiological Society. 1964. Pp. viii + 399. 55s.

A serious interest in the nature of substances that could excite pain fibres in the periphery first arose as a result of the experiments carried out by the late Sir Thomas Lewis, the earlier ones being summarized in "The blood vessels of the human skin and their responses." He postulated the presence of chemical mediators to explain ischaemic pain and the itch of the allergic response. Professor Keele and Dr. Armstrong have been prominent amongst those who have continued this type of study and in their very readable book present a comprehensive account of the substances known to produce pain together with the methods used for testing and evaluating them. These include the effect of hydrogen ions and other inorganic substances, acetylcholine, histamine, serotonin, and the kinins and the effects of stings and venoms. The book ends with a valuable summary of clinical pain having its origin in the periphery. This is an invaluable source book for those who wish to know the features of pain produced by chemicals acting peripherally.

A. S. V. BURGEN.

Disorders of Langugage: A Ciba Foundation Symposium. Edited by A. V. S. de Reuck and Maeve O'Connor. London: Churchill. 1964. Pp. xii + 356. 50s.

The now fashionable view of aphasia as practically unexplored from the point of view of the psychologist, contrasts strangely with the massive documentation of this common condition by successive generations of neurologists. This paradox is the result of contrasting attitudes. The doctor, unaccustomed to being contradicted, feels free to draw empirical observations into an elaborate theoretical framework. The psychologist, reacting against methodological deficiencies, assumes that these viewpoints are not only unsupported but therefore incorrect. This iconoclastic attitude detracts from the objectivity of some recent studies performed by psychologists.

The Ciba Foundation's initiative, at Lord Brain's suggestion, in convening the meeting reported in the book under review, arose from the wish to reconcile contrasting attitudes and misunderstandings by means of multidisciplinary personal confrontation. Workers in several fields relevant to the study of language presented communications which were then made the subject of discussion.

The debate resolves itself into two main problems. What new methods can profitably be introduced into aphasia research, and in the meantime, what can be concluded from existing results? As regards the first question, a number of papers were presented by workers in normal psychology, phonetics and linguistics, in the hope that the application of their work to the aphasia problem, would become apparent to those with experience in this field. As regards the second, a number of established authorities on aphasia presented statements of their findings and conclusions.

Several communications (Jakobson, Luria, Hécaen and Angelergues) presented classifications substantially similar to those traditionally accepted, and the last mentioned workers effectively used statistics in relating their findings to the presumed localization of lesion in an extensive series of patients. Bay, however, raised the more fundamental question, subsequently skilfully evaded by the other contributors, as to whether aphasia may not more simply be regarded as the result of undimensional degradation of language, coincident with various degrees of visual, auditory, motor and intellectual deficit making for a constellation of distinct clinical appearances. The occasional preservation of intelligence, assessed by non-verbal means, in patients with severe aphasic disorders was documented by Zangwill. Stengel reviewed the effect of mental disorders on verbal behaviour. Alajouanine and Lhermitte commented on the non-verbal fringe of communication disorders.

In contrast to the traditional approach of these authors were reports of studies utilizing novel methods. Milner's report of the effect of intra-carotid amytal injections on speech resolves the long lasting controversy on the lateralization of language functions in left-handers. Everyone was right. Language may be represented in the right hemisphere, the left, or bilaterally. This was the outstanding original contribution to the meeting. Howes discussed his application of word frequency analysis to an aphasic population. The results, largely negative, permit the conclusion that in respect of the aspects of language studied, aphasics differ only quantitatively from normal subjects. It would be incorrect to generalize this conclusion to the many aspects of aphasia not studied by Howes. Ross presented a detailed phonetic transcription of a case of jargon aphasia studied by Clarke. This approach will become significant when it is applied to the resolution of specific problems in relation to the phonetic characteristics of aphasic speech.

The remaining papers describe work not directly related to aphasia, which it was hoped would inspire new ideas among the aphasia experts. Broadbent reviewed recent advances in psychology of speech. Goldman-Eisler summarized her studies of the implication of hesitation pauses in speech and Neff presented his elegant studies of temporal pattern discrimination in animals.

The symposium illustrates the very real difficulty encountered by established workers in a field in reorientating themselves, when they are not really convinced of the shortcomings of their existing orientation. In the discussions that followed, the aphasia workers gave little sign of their willingness to apply new methods to their research.

We are left with a series of useful summaries from authorities in aphasia research, and with a series of discussions, which, tedious in length and trival in detail, are an object lesson in non-communication. They should have been severely edited or omitted altogether.

M. KINSBOURNE.

Critical Essays on Psychoanalysis. Edited by Stanley Rachman. Oxford: Pergamon Press. 1963. Pp. xiii + 284. 45s.

This book brings together a number of previously published papers, some of which are quite well-known. It begins with two that give personal accounts of analysis by Professors Landis and Boring. Under the section heading "General Evaluation of Psychoanalysis," there are three papers: "The Great Psychiatric Revolution" by Dr. P. B. Bailey, "Psychoanalysis—Myth or Science" by Professor Eysenck and "An Introduction to the Principles of Scientific Psychoanalysis" by Dr. A. E. Ellis. The next section is called "Aspects of Psychoanalytic Evidence," and contains two papers: the oft-quoted article by Dr. H. Orlans on "Infant Care and Personality" and the recent paper by Drs. Wolpe and Rachman containing a critique of Freud's case of Little Hans. The next section, called "Clinical Aspects of Psychoanalysis," contains three papers: "The Prognosis in Unpsychoanalysed Recovery from Neurosis" by Dr. Wolpe, "The Aetiology and Treatment of Children's Phobias: A Review" by Drs. Rachman and Costello and "Multiple Personality" by Professor Thigpen. The last section, called "Prospect," contains a paper by Professor Dallenbach "Phrenology versus Psychoanalysis." I think Dr. Rachman has done us all a service in collecting these papers and so making them much more accessible.

This is not an easy book to review briefly with justice. The reason is that the papers contain a number of different sorts of criticisms of analysis; and the character of any one of these criticisms is a complicated business that needs time and care to unravel. Thus, Ellis criticises analysis on the main grounds that analysts do not have the attitude of scientists to the subject, and that analytic theory is not scientific in various ways. Bailey argues that it is difficult to say in what ways the great psychiatric revolution has been an advance, in spite of all the excitement. "Revolutions do not necessarily bring progress. . . . Are the mental patients better off as a result of the great psychiatric revolution?" Eysenck compares analysis to a myth, and argues that "psychoanalysis is a science, subject to the usual dictates of scientific argument and scientific evidence, or it is nothing." Orlans reviews some of the empirical evidence bearing on the view that various features of infant care determine adult personality, and comes to his well-known, negative and sceptical conclusion. Wolpe and Rachman examine Freud's account of Little Hans, and conclude that this case provides "no direct confirmation by direct observation for any psychoanalytic theorem, though psychoanalysts have believed the contrary for 50 years." Wolpe argues that the facts of recovery from neurosis of unanalysed patients "contradicts the psychoanalytic expectation of inferior durability of recoveries obtained without psychoanalysis and does away with the chief reason for regarding analysis as the treatment of choice for neurotic suffering." Rachman and Costello reject the analytic theory of children's phobias as inadequate, and offer a Behaviour theory instead. Thigpen explains why Cleckley and himself have never given a "dynamic" (or "more or less Freudian") explanation or interpretation of the case of Eve. He could provide such an explanation if he were prepared to use methods such as Freud employed in his study of the Wolf man; but these methods are clearly unscientific, and are almost identical with the methods of Fodor and Peerbolte, who claim to have discovered that patients can be traumatized during fetal life by the father's penis during parental sex intercourse. Dallenbach draws a parallel between Gall and Freud, and suggests, without actually saying so, that Freud's niche in history will become "a roost for charlatans," just as Gall's became in time.

How cogent and important is this criticism of analysis? What does it add up to?

(1) It is no doubt perfectly correct that, in a very well-known sense of the word "scientific", analytic theory is not a scientific theory, that analytic method is not a scientific procedure, and that analysis has not been shown to work as a therapy. In all these respects, the argument of the book is quite right. It *may* also be correct that analysts themselves do not possess a scientific attitude to the data of analysis, and the problems these raise. I say "may" because this sociological generalization about analysts is a difficult one to establish, and I am not sure that any sociologist has done the necessary work on this professional group to establish it. But if analysts do not possess this scientific attitude, then again the book is quite right to point this out.

(2) The book also appears to take the general line that analysis is an irrational enterprise, wholly devoid of justification and unworthy of support. What backing does the book provide for this contention? The general argument seems to be that, because analysis is an unscientific business, it follows that it is also an irrational and unworthy enterprise. But from the fact that analytic theory and procedure are not scientific, it does not follow in the least that analysis is an irrational enterprise wholly devoid of justification. Ordinary academic history is hardly a scientific enterprise; but it is not on this account an irrational one. If analysis is an irrational enterprise—a myth, the current phrenology, and so forth—then this has to be established independently. This book hardly begins to do the job. So the reader should not allow himself to be trapped into supposing that, because the Volume shows that analysis is an unscientific affair, it thereby also shows that it is an irrational and unworthy one.

(3) This same mistake detracts from the value of the book in yet another way. The book is obviously concerned to do propaganda for the value of the scientific attitude, and the importance of pursuing scientific methods in the field of personality theory and therapy. This is plainly "a good cause," and a desirable thing to support. No doubt, such propaganda is especially desirable and needed in America, where the popular and academic cult of analysis is widespread. Unfortunately, the general value of the book is seriously weakened by the fact that the propaganda is exaggerated. It is exaggerated because, in arguing for science, it commits the mistake we have just mentioned, and assumes that unscientific theories and methods are irrational and unjustified. In consequence, it overlooks the value and legitimacy of non-scientific theories and procedures, and ties up its defence of science with views about analysis that are false or extremely unplausible. For example, Freud's procedure with little Hans and the dreams of the Wolf man were obviously quite unscientific. But it does not follow from this that the interpretation of the material thrown up in the course of an analysis is always a wholly arbitrary business, and can lend no rational support for this or that analytic view of human development and functioning. Likewise, it does not follow that, because the theory of psychoanalysis is an unscientific affair, it has made no contribution whatever, and is of no importance at all to psychology and psychiatry. These contentions are false or very unplausible. Clearly, it spoils one's case for science if one links it with dubious theses about analysis and seriously underplays the value of the theory and methods of analysis.

(4) At this intellectual juncture what we require are a number of constructive enquiries. For example, the objective examination of psychoanalytic methods (the autobiographical reports of patients, even though these be by eminent psychologists, hardly help us much); the investigation of the logic of psychoanalytic and psychotherapeutic interpretation, to see what can be legitimately based on this widespread practice; the study of analysts as a professional group; the examination of the logic of psychoanalytic theory to bring out the likenesses and differences from scientific and other theories; the logic of explanations of the same data by analytic and Behaviour theory; the persistent attempt by psychologists to discover empirically what there is of value in the generalizations and concepts of analysis. For some of these studies it is necessary to obtain the co-operation of analysts themselves. Eysenck once wrote: "There is much that is supremely important in Freud's contribution to psychology, but there is also much that is bad" (*Uses and Abuses of Psychology*, Chap. 12). Unfortunately, he did not go on to elucidate what this much is that is supremely important in Freud's contribution. If we could do this, we should do something to resolve the current confusions about the character of Psychoanalysis. On the other hand, if we confine ourselves to emphasizing the unscientific character of analysis (which is what this book does to a large extent), we shall probably not do much to resolve the present debate on the subject.

B. A. FARRELL.

Advancing Psychological Science. Vol. 1. *Philosophies, Methods and Approaches.* Pp. xiii + 122. *Vol.* 2. *Research in Perception, Learning and Conflict.* Pp. xii + 177. *Vol.* 3. *Research in Developmental, Personality and Social Psychology.* Pp. xii + 190. Edited by Fillmore H. Sanford and E. John Capaldi. Belmont, California: Wadsworth Publishing Co. Inc.; London: Prentice-Hall International. 1964. 16s. each.

This is yet another Paperback series of selected readings, intended to supplement undergraduate psychology texts. Vol. 1 dazzles us with excerpts from Bacon, Whitehead and Oppenheimer, presumably to set the tone, but tails away rather sadly with a piece on the professional psychologist as an expert witness. In Vol. 2, Perception is represented mainly by Eleanor Gibson, Learning by K. W. Spence and Janet Taylor, and Conflict by Neal Miller. Vol. 3 is a little more wide-ranging, taking us from Harlow and Lorenz to Adorno, Sherif and Asch. Although some of these excerpts are useful, it is difficult to believe that this style of publication advances anything very much, least of all education.

O. L. ZANGWILL.

Information Transmission. By Elwyn Edwards. London: Chapman and Hall. 1964. Pp. 134. 15s.

This book is addressed primarily to the "large number of students in the behavioural sciences, who lack training in mathematics and are consequently at a disadvantage when attempting to come to terms with ideas which are essentially mathematical." The author makes it explicit that the book is not intended as an exhaustive review and that "nothing original will be found in these pages." The quotations are from the preface, the first paragraph of which poses the pertinent question of why there should be a need for "an introductory exposition of the subject in the form of the present book"? The short answer to that question is that there is no need for *such* an introductory text in 1964. What is needed now is something which puts information theory in perspective and leads on to decision-making theories and other mathematical models.

It is inevitable that one should compare the present book with Attneave's *Application of Information Theory to Psychology*, published in 1959 and still easily available though at twice the cost of the book under review. The outcome of the comparison is not too favourable for the recent addition. For in his little book in 1959 Attneave kept apart the separate aspects of information theory and managed at the same time to convey something of the excitement, the scepticism, and the disappointments encountered by psychologists who had attempted to apply information theory during the preceding 10 years.

Dr. Edwards is undoubtedly right to assume that many who enter psychology are justifiably put off by an array of summation signs and subscripts, and so there is some virtue in his second and third chapters which deal with basic mathematics, binary arithmetic and neural nets. But it is this very population which is likely to be misled by the rest of the book. The distinction between the use of information measures as a metric, of information theory as a hypothesis generating framework, and as a possible model of neural processing is blurred. To me, too, Dr. Edwards appears to perpetuate a tradition which, in seeking for simple solutions, put a false emphasis on the noise free channel. The present state and likely future development of computers has almost annulled the traditional escape clause that the nervous system is complex beyond the powers of comprehension let alone simulation. We have come to accept not merely that the central nervous system is abominably noisy, but that it is possible to conceive of solutions which would lead to "reliable computation in the presence of noise."

J. A. LEONARD.

Complex Human Behavior. By Arthur W. Staats and Carolyn K. Staats. New York and London: Holt, Rinehart and Winston. 1963. Pp. xiii + 546. $7.50.

The authors analyse simple behaviour in terms of reinforcement, much as Skinner does but with some additions, and they apply the results of this analysis to a wide range of complex human behaviour, citing relevant experiments wherever possible.

The introductory chapters are concerned with methodology and with principles of behaviour that are demonstrated by conditioning experiments, especially those of the operant variety. The next chapters take up the process of verbal learning and the relationship of verbal to non-verbal behaviour, going some way towards bridging the gap between the rats who press levers and the people who provide them with the opportunity to do so. In the chapters that follow, human personality, motivation and social interaction are examined and the book ends with the topics of child training, educational psychology and the behaviour disorders.

Obviously the book covers a wide field and its title is well justified. The greater part of it would seem to suit advanced students rather than beginners but the introductory chapters pre-suppose little knowledge and the later chapters are self-contained. While the original contribution of the authors should not be underestimated the value of the book as a whole is that it provides the reader with an opportunity to see where the analysis of behaviour has got to and what the growing points are. It also exposes gaps. In spite of the effort that has been made throughout to go beyond verbal analyses and everyday illustrations to the results of experiments, the empirical content of some of the later chapters remains low.

Criticism of the general approach, behavioural analysis, would not be appropriate, especially since the book provides a documented case for its fruitfulness, but even the most sympathetic reader may find it difficult to be enthusiastic about some of the do-it-yourself philosophy included in the first two chapters. The handling of some topics produces an uneasy feeling that important problems are left lurking in the background. For example, Staats and Staats rightly say that Köhler was wrong to assume that the past experience of his chimpanzees did not contribute positively to their ability to solve problems. They then provide an analysis in terms of reinforcement, useful in itself, but taking no account of Schiller's work and ignoring the difficulty that in the case of the chimpanzee a little reinforcement seems to go a very long way towards establishing the components of problem-solving behaviour. Similarly, the treatment of the acquisition of language seems to provide part of the explanation without recognizing that the other part is missing. It explains human communication by referring to behavioural processes that occur in rats. But rats do not speak and human beings do. A behaviouristic analysis that does not include capacity variables is necessarily incomplete as Hull and Tolman recognized.

While many criticisms might be made, the book must be judged to be highly successful in doing what it sets out to do, and it can be strongly recommended for providing a comprehensive well-integrated account of the present state of behaviouristic research outside the walls of the shuttlebox or operant conditioning chamber. The reader gains an impression of vigorous progress over a wide front and the publishers use of the adjectives "fresh," "vital," "exciting," has some justification.

R. L. REID.

Social Behavior and Organization Among Vertebrates. Edited by William Etkin. Chicago and London: The University of Chicago Press. 1964. Pp. viii + 307. $7.50. 56s.

This book contains ten chapters, written by six authors. Five of them are written by specialists on particular aspects of social behaviour—D. E. Davis on physiological aspects of aggression, F. A. Beach on reproductive behaviour, D. S. Lehrman on the control of reproductive cycles, N. Tinbergen on the evolution of signalling devices, and J. P. Scott on the effects of early experience. Each of these chapters provides an excellent summary of its field, incorporating recent data but written in a non-technical fashion. The remaining five chapters, written by the editor, are somewhat less satisfactory. Covering topics which range from "neuroendocrine correlation" to social organization, the treatment is inevitably more superficial. The discussion of motivational theory, for instance, is at least controversial: many would consider it dated. Taken as a whole, however, the book provides a useful introduction to the analysis of social behaviour.

R. A. HINDE.

Personality Assessment: A Critical Survey. By P. E. Vernon. London: Methuen. 1964. Pp. ix + 333. 42s.

This book is a valuable successor to Professor Vernon's *Personality Tests and Assessments* and *The Structure of Human Abilities.* Its contents follow the title closely and it should prove extremely useful to students, in the wide and accurate review of many important issues that it provides, and to psychologists using their professional knowledge in real life situations—attempting to select, assess, or predict the actions of other people. To the experimentalist or researcher in the field of personality it is less likely to be of use.

Throughout the text, sound commonsense and a very balanced and informed judgment have been applied to a great many topics. This is particularly the case where the issues involved are "basic": less so when detailed criticism is necessary. This last flaw is demonstrated in a tendency, here and there, to over-simplify and over-generalize. For example, the author writes of projective techniques. . . . "Though not normally fakable nor liable to ordinary 'social desirability' effects. . . ." Few sophisticated users of such techniques could agree. Again, where specifically experimental results with animals are cited, we have such sweeping statements as: "Disinterested concern for the distress of other members can be observed with porpoises as well as chimpanzees." (p. 52) or, and here your reviewer is lost, "Skinner's discovery of the greater stability (resistance to extinction) of habits such as bar-pressing in rats, through applying intermittent schedules of reinforcement, can be regarded as a major contribution to human dynamics. For example, it helps to explain why the spoiled or over-protected child who is too frequently reinforced shows less tolerance of frustration than the occasionally rewarded" (p. 93).

These are, however, very occasional lapses, and they count for little against the very wise comments, based on a wide and sensible appreciation of the literature, that are found throughout the text. The author is highly sophisticated in the nuances of "set," meaning, preconceptions in test takers, etc., that bedevil the major theories and methods involved in the assessment of personality. He is throughout refreshingly concerned with what people actually *do* in such circumstances. The book is excellent on problems of measurement and has most lucid chapters on the concept of validity, traits and factors, and on the findings and concepts of depth psychology. I have never read a more balanced consideration of the respective merits and demerits of the ideas of depth psychology (including the effectiveness of therapies) and those of more "objective" approaches. Professor Vernon can be critical of both depth ". . . there is more than a suspicion that *ad hoc* hypotheses are being fabricated to plug the gaps whenever the initial theory is not borne out," and of factor analytic approaches: "Despite the factorist's rigorous methodology, he seems apt to indulge in theoretical speculations which merit at least as severe criticism as he himself applies to clinical theories. He sets out, in praiseworthy fashion, to employ operationally defined constructs: for example, extraversion-introversion (or any other factor) *is* the score on a properly weighted cluster of intercorrelated tests. But he soon begins to reify his factors in much the same way as the layman or non-factorist reify their traits, complexes, etc., and to assume that it determines behaviour in all kinds of daily life situations, on very little or no evidence" (p. 193). This is well worth the saying.

Projective techniques come very badly, as predictive instruments, out of the author's scrutiny. But there is little or no discussion of the specifically experimental use of such tests where such prediction has on many occasions been adequately enough achieved. He himself comes down tentatively in favour of further work with somatotyping, cognitive tests of perception, rating methods based on direct observation of behaviour, and situational tests. Throughout, a practical interest underlies the writing—often with specific counselling or selection needs in view.

The text is very broken up in that there are literally hundreds of headings and subheadings. This does not make for ease of reading at one sitting but increases the value of the book for student reference. It should be invaluable to any student who chooses to refer to references cited in the text. Some five hundred of these appear in the bibliography and are exceptionally well chosen.

S. G. LEE.

A Model of the Brain. By J. Z. Young. The William Withering Lectures. London: Clarendon Press, Oxford University Press. 1964. Pp. vii + 348. 35s.

This book records a determined attempt to discover physical principles to explain animal behaviour, and to make use of concepts drawn from the subjects of computer design and control engineering. As the author says in the Introduction, the book does not offer a solution, but provides data for those seeking one. Concentrating on the octopus, half of the book is devoted to anatomical and psychological data which must certainly be taken into account. The latter provides the reader, in a compact form, with key results obtained from long and patient experiments. Of particular importance is the discovery that generalization of pattern recognition in the octopus occurs only within the experienced range, and not beyond it.

The attempt to relate anatomical and psychological data is like trying to span the Straits of Dover in a single arched bridge. Intermediate footings are required in Biochemistry, Neurophysiology and Engineering. Consider Sutherland's hypothesis that retinal activity in octopus is summated along two roughly perpendicular directions; this could be checked rapidly by a neurophysiologist using microelectrode techniques. However, the charge that this has not been done must be levelled at British Physiology rather than at the author, who will be well aware of the need for such work.

The engineering pillar of the bridge has been considered in great detail; here the earlier chapters on concepts will be found more rewarding than that on the model of W. K. Taylor; this model is perhaps too specific and open to question for arch building just yet.

The author says "If this book gives anyone the opportunity to play with ideas about the brain it will have served the function of a good toy." He is too modest; anyone who has the courage to work on the problem of the brain must read this book; he will be deeply in the author's debt for so clear a statement of some of the relevant facts.

A. M. UTTLEY.

A Hundred Years of Psychology. By J. C. Flugel. Revised by D. J. West. London: Methuen (University Paperbacks). 1964. Pp. 394. 21s.

In 1933, J. C. Flugel wrote a modest volume in the "A Hundred Years" series which he confessed was "deliberately constructed on more impressionistic and less systematic lines" than the works of Boring and Murphy, which he hoped his "inadequate and somewhat sketchy story" would lead the reader to consult. Flugel relied heavily on these works (plus Brett and Warren) for much of his information and many of his judgments. His independent evaluations have dated rapidly, e.g. McDougall's was the most successful neurological theory that had ever been propounded, and Spearman was the person to whom psychology was most indebted for knowledge of the structure of the human mind. It was not an inaccurate or incompetent work, but it was pithless. The text of the revised edition of 1951 was unaltered, and a chapter on "Developments 1933–47" was added which was dominated by the war which had just ended and made no attempt at comprehensive treatment. The new edition leaves the text to 1933 unaltered and contains a revised chapter on "Developments 1933–63" by D. J. West. This is certainly an improvement on Flugel and fulfils its own modest claims: "Within the compass of the supplementary section, one can do no more than list some outstanding trends and refer to a few researches chosen almost haphazard [sic] according to one's particular interests, prejudices and limitations."

The soft cover edition costs 21s. (a hard cover edition is available at 30s. from Duckworth). It is printed on paper which glares in any light, and this reviewer found it an act of conscience to read the whole thing through. Given the standard set by Passmore's *A Hundred Years of Philosophy*, it is difficult to understand why the publisher has allowed this volume to be reissued. R. M. YOUNG.

A Short History of British Psychology, 1840–1940. By L. S. Hearnshaw. London: Methuen. 1964. Pp. xi + 331. 35s.

Until the publication of this book, no work devoted exclusively to the history of British psychology had appeared since 1874, and Ribot remains the authoritative expositor of the Mills, Bain, Spencer and Lewes. Professor Hearnshaw's book aims "to trace the development of psychology in Great Britain from the middle of the nineteenth century to the time of the Second World War," developments which have "transformed a largely abstract philosophical study pursued by a small number of individuals into an independent, if rudimentary, science, with many practical applications and the beginnings of a professional organization." . . . "It is only an outline, and aims in the first place to bring out broad trends rather than to introduce an excessive load of minute and technical detail." Professor Hearnshaw begins his account with Bain, which leaves a study of the works of Hobbes, Hartley, Erasmus, Darwin and Brown an important desideratum, though Warren will continue to serve as a useful source in the interim.

The present work consists of a series of independent essays of varying lengths. Many of these are biographical and intellectual sketches of important figures and their milieux (at the level of expanded *Dictionary of National Biography* articles). These sometimes lay heavy emphasis on "firsts" and debts of gratitude, e.g. "British psychology may justifiably feel proud and grateful for the support of so great a scientist and so great a man as Sir Charles Sherrington." These examples of the "Whom to worship?" view of the history of science are complemented by lucid critical essays on Ward, Stout, McDougall and Spearman. The national scope of the book allows treatment of some unfairly neglected figures, e.g. William Carpenter, Thomas Laycock, Henry Maudsley, G. H. Lewes, Hughlings Jackson, Ferrier, Bastian, Head, Lubbock, though treatment of them is necessarily brief.

Professor Hearnshaw has a broader appreciation of general history than the standard histories of psychology, and the best parts of his book are the topical ones dealing with aspects of psychology which are related to social history—legislative changes, social and abnormal and applied psychology, development of laboratories and associations. The book contains an immense amount of information about the history of the *discipline* of psychology in this country which is not readily available elsewhere. The sections on philosophical climate and background are less successful.

The book has the merit of having been written almost exclusively from a wide variety of primary sources, many of which have not appeared before in the secondary literature. It also refrains from perpetuating the dubious judgments of predecessors who often have not read the primary sources. It is also comprehensive: I can recall no noteworthy figure or work falling within the scope of the book that is not at least mentioned. This is achieved with a charming and clear style coupled with a rather engaging trace of whimsy and taste for quaint details which is marred only by an irritating over-use of exclamation. The critical bibliography is an excellent introduction to the literature.

In conclusion, this is a useful beginning and a convenient summary which admirably fulfils its own limited aim. (One hopes that Professor Hearnshaw will expand some of his essays into critical studies for the new *Journal of the History of the Behavioural Sciences.*) One need not hesitate to recommend this book to students for general background or to consult it to satisfy one's own curiosity on a given point. On the other hand, it is not a work of intellectual history, nor is it likely to raise basic questions. It is more informative than provocative and conveys little sense of persisting fundamental problems or of the continuity of scientific ideas and influences. If the history of psychology is to contribute directly to the science of psychology it must provide a perspective on current problems which transfers readily from one's leisure reading to one's study and ultimately to the laboratory. R. M. YOUNG.

Psychophysical Relations in the Perception of Velocity. By Madjid Mashhour. Stockholm Studies in Psychology 3. Published by the University of Stockholm. Almqvist and Wiksell. 1964. Pp. 176. Sw. kr. 22.

This monograph falls into two parts. The first inspects the validity of the methods of magnitude and ratio estimation and the usefulness of the application of power functions to biophysics, including psychophysics. The second part presents experiments on the nature of the perception of velocity. Tests of the validity of Brown's Law—that the relation between subjective space, time and velocity is the same as that between the corresponding physical variables—and of the applicability of Pieron's Law (1914)—that RT is a decreasing function of stimulus intensity—and an investigation of the role of eye movements, all point to the direct perception of velocity. The clarity in which the results are presented makes the study the more convincing. C. F. LEGG.

Flavour, Taste and the Psychology of Smell. By Warren Gorman. Springfield: Thomas. 1964. Pp. x + 106. $5.50.

The fascinating problems of taste and smell in their biological and behavioural context are all but neglected. This rather garrulous little book, which ranges in 100 pages from baked beans to the family history of the Darwins, serves to remind us of a large and important field awaiting exploration. Perhaps the author is right in the emphasis he places on the repression to which the topic is subject. But the reminder is an expensive one at £2 a time. R. C. OLDFIELD.

Learning: A Survey of Psychological Interpretations. By Winifred F. Hill. London: Methuen (University Paperbacks). Pp. x + 227. 1964. 9s. 6d.

A clearly written and not uncritical account of theories of learning in the American tradition. Although written principally for teachers, this book should be of distinct use for elementary psychology courses and can be recommended. O. L. ZANGWILL.

Statistical Concepts: A Program for Self-Instruction. By Celeste McCollough and Loche Van Atta. London and New York: McGraw Hill. 1963. Pp. xv + 367. 46s. (Paperback Edition 31s.)

This is an introductory text on statistics, programmed for self-instruction along the lines of Holland and Skinner's *Analysis of Behaviour*, and covering the usual ground (frequency distributions, chi-square, t-test, etc., ending with a lesson on analysis of variance). It is designed for undergraduate psychologists without mathematics, and appears to concentrate upon conveying insight into the use and nature of statistics, rather than upon providing a guide to the practical application of particular statistical tests. It thus tackles the most difficult task in the teaching of statistics; it is easy enough for a mathematician to understand the nature of statistics, and also for the non-mathematical research student, armed with a suitably indexed textbook, to look up and apply designs and tests when the need arises, and thereby slowly, but relatively painlessly, to gain insight into what is being done; but it is not easy to convey understanding without mathematics or the spur of practical necessity, and any new approach, particularly one as promising in other fields as a programmed text, is to be welcomed.

The present book would therefore be of interest, as a potential alternative to the conventional textbook, even if it were bad; and it is certainly not that, though it is not without faults. For instance, the answer in a frame is too often determined, or nearly determined, by the context (thus, on page 8, " . . . populations that are ——— from one another" obviously has the answer "different"); the elucidation, on p. 99, of the independence requirement for the chi-square test (surely a stumbling block for most teachers of statistics to non-mathematicians) is disappointingly inadequate; and the way in which the authors use rank correlation to explicate product-moment correlation seems likely to be a source of confusion. Such criticisms, however, are outweighed by the smooth and professional competence of the book as a whole, and it is worth the careful attention of all teachers and students of elementary statistics.

A. J. STILL.

Experiments in Behaviour Therapy. Edited by H. J. Eysenck. Oxford: Pergamon Press. 1964. Pp. x + 558. £5.

This volume brings together 42 papers on the methods and theories of "behaviour therapy." A few were written specially; most have been published elsewhere. More than a third provide reports on the treatment of one or two cases. About a fifth report on a series of cases. Some of the remainder are reviews of methods and their applications; others discuss theoretical points.

Very few, if any, of the reports on one or two cases are satisfactory, although they may be of interest as illustrations of the ways in which the methods were applied. Most of the disorders treated are relatively common, e.g. indecent exposure, transvestism, homosexuality, hysterical blindness, food fad, tics and special phobias, and it is to be regretted that the authors did not gain a wider experience before making claims for their methods on the basis of levels of recovery often achieved by less time-consuming methods. In some cases the discussion of the results omits important details, e.g. whether the patient was able to leave hospital or make a return to work. Some people will think the aversion methods, in particular, uncongenial, to put it mildly. In some instances the author, although not the Editor, may be excused by his lack of experience in the treatment of patients.

The experimental design of a study of "desensitization" of snake phobias in a substantial group of experimental subjects (College students) was a conventional one and may be accepted as satisfactory. On the other hand, only one of the several papers on the results of treatment in a series of patients reports an attempt to validate the results experimentally, by comparing them, for instance, with those obtained in a control group, and this attempt had serious and avoidable defects in its design. It would be surprising to anyone forgetful of the parable about motes and beams that Professor Eysenck, who has castigated psychoanalysts for their failure even to attempt to validate their results experimentally, should show such indulgence to authors who have squandered at least as good opportunities to make an attempt. Psychoanalysts, moreover, have tended to be more modest in their claims. Yet there are indications in Professor Eysenck's apologetics in his introduction that he is beginning to understand that the failure of psychoanalysts to provide the evidence he repeatedly asks them for is due as much to the difficulties of the problems as to their wrong-headedness.

The pity is that Professor Eysenck's scientific grandiosity tries the patience of readers as of reviewers, for much of what he has to say is of interest. His definition of behaviour therapy, for instance, as "the attempt to alter human behaviour and emotion in a beneficial manner according to the laws of modern learning theory," would be regarded by many as a good shot at a definition of psychotherapy. Psychotherapists would probably want to shift the emphasis slightly and say something about creating conditions in which behaviour tendencies and attitudes may be modified through experience. They would be less confident than Professor Eysenck about the modernity of their learning theories. Indeed, one can no longer criticize him for being old-fashioned, as one used to do when he relied heavily on Pavlov's theory. Now he is keeping up well with the Joneses, and expresses regrets that in a recent book "the Hullian viewpoint was perhaps stressed a little too much." He has tried to "redress the balance by incorporating much material relating to operant conditioning."

He still has strange ideas about what other people—especially psychoanalysts—do and think. Three-quarters of a century ago, Freud discussed, admittedly in the terms of a learning theory which was old-fashioned even then, although anticipating Thorndike in some respects, the ways in which neurotic behaviour was acquired through experience. As Freud recognized, the difficulty then was, as it still is, that neurotic behaviour does not appear to be modified by new experience to the degree that the laws of learning theory suggest that it should be. Freud gave reasons why this might be so, not all of which are now accepted. In his paper on "spontaneous remission and transference," Professor Eysenck has got a little way along the winding path Freud followed, and stands on the threshold of great new re-discoveries. It should however be noted that "when it is stated that behaviour therapists reject the notion of transference, what is meant is a rejection of the speculative theory and not of the facts themselves" [*sic*]. Further elaboration of modern learning theory is required if it is to explain neurotic as well as normal behaviour. With this view Professor Eysenck would, I think, agree.

D. RUSSELL DAVIS.

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STUDIES ON SENSORY FEEDBACK: III
THE EFFECTS OF DISPLAY GAIN ON TRACKING PERFORMANCE

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Instrumentation is described which permits study of the effects of different forms of visual feedback display on the patterns of fine movement obtained from the extended human index finger when the subject is attempting to keep his finger at a fixed point in space. The task is a compensatory tracking task in which the only source of input to the system is the subject's own finger movement. The effects of increasing the gain (or amplification) of a proportional error signal on the pattern of finger movement was studied. Gains of 1, 2, 4, 10, 20 and 40 were studied with a group of 24 subjects. Increasing the gain of a proportional error signal resulted in a marked improvement in the ability of subjects to maintain their extended finger at a fixed point in space. As the gain of the error signal was increased, the subject's high-amplitude, low frequency errors were reduced, and there was a progressive appearance of high-frequency activity of low-amplitude, more accurately centred about the reference position in space. A total off-target area measure (integrated absolute error) showed marked decrease in scores as the amplification of the error signal was increased from 1 through 10. Beyond this gain there was no appreciable additional improvement in motor control, however no degradation of control was noted to characterize the group performance.

Exploratory studies were undertaken to permit comparison of the effects of increasing the gain of a proportional visual display with the effects of increasing the gain of non-proportional visual and auditory displays. An increase in the dominant-energy frequency was noted as the error signal gain was increased, independent of whether a proportional visual, or non-proportional visual or auditory display was used. This observation suggests that common mechanisms mediate the processing of the gain parameters of feedback displays, in some measure independent of the display form or the sensory modality used for presentation.

INTRODUCTION

A. The experimental study of sensory feedback influences on motor control

Experimental studies on the relationships between sensory feedback and movement have their embryonic beginnings in the work of François Magendie (1822) and Sir Charles Bell (1811, 1826) on the functions of the spinal roots. Clear demonstration of the profound deficit of voluntary motor control following deafferentation was provided by the remarkable experiments of Claude Bernard (1858) and Sherrington and Mott (1895). These early experiments on the effects of dorsal root section on movement have been corroborated and extended by Lassek (Lassek, 1953*a* and *b*, 1955; Lassek and Moyer, 1953), and have now been combined with behavioural studies which have given support to the original observation that the elimination of sensory feedback from an extremity results in marked impairment of voluntary movement. The findings that conditioned responses survive, or are readily re-established following deafferentation (Knapp, Taub and Berman, 1958; Gorska and Jankowska, 1959, 1960) suggest that the study of the role of sensory feedback in the initiation and control of movement may yield criteria for new organizations of classes of motor activity (Chase, 1963).

Previous work in the area of sensory feedback studies has concerned the effects of delayed sensory feedback on motor control (Chase, Sutton and Rapin, 1961; Chase, Harvey, Standfast, Rapin and Sutton, 1961; Chase, Rapin, Gilden, Sutton and Guilfoyle, 1961; Chase, 1963). These studies served to show that the temporal aspects of sensory feedback generalize across movement categories and sensory modalities.

We now wish to turn our attention away from the questions of the time relationships between sensory feedback and motor response, and focus upon the effects of differences in the *form* of undelayed sensory feedback on patterns of motor control. It is hoped that the study of relationships between the form of sensory feedback and the form of motor activity will yield generalizations similar to those which have emerged from the studies of time relationships between sensory feedback and motor response. Such generalizations are not only of use in the progressive understanding of mechanisms underlying the initiation and control of movement, but also serve to define basic brain mechanisms for the generation and processing of information as such (MacKay, 1955, 1961; Chase, 1963). It is to these ends that the instrumentation system which will next be described, and the experiment which will provide the main body of this paper were designed.

B. *Outline of instrumentation*

We have designed a new instrumentation system to provide broad capabilities with respect to the forms in which sensory feedback accompaniments of motor activity can be presented to the nervous system. The motor activity selected for study was index-finger movement. There were several reasons for this selection: (1) It is easy to design a mechanical transducer system which will permit recording of extensor and flexor activity about the metacarpophalangeal joint. Such a system permits speculation about antagonist muscle systems and spinal reflexes which are more readily schematized than would be the case for more complex movements, e.g. those taking place about a ball-and-socket joint. (2) The preponderance of extension and flexion activity for this system permits us to record movement in a single plane, and thus to generate a single voltage which provides information about the position of the finger in space with respect to some reference position. (3) The peripheral location of the neuromuscular and mechanical components of the system for finger movement permit alterations in sensory feedback from the extremity under study. Digital nerve blocks and changes in temperature between antagonist muscle groups serve as examples of such alterations. (4) A wide spectrum of neurological diseases which result in impairment of motor control involve the movements of the upper extremity. We can therefore study the patterns of utilization of sensory feedback with respect to a motor control function which reflects a specific pattern of pathology, and compare these findings with those obtained from parallel studies done with normal subjects.

A large number of different transducers have been used in the study of patterns of finger and hand movement in human subjects. Tambours (Schäfer, Canney and Tunstall, 1886; Binet, 1920), strain gauges (Tuttle, Janney, Wilkerson and Imig, 1951), accelerometers (Marshall and Walsh, 1956; Marshall, 1961; Brumlik, 1962), photoelectric cells (Cooper, Halliday and Redfearn, 1957; Halliday and Redfearn, 1956; Van Buskirk and Fink, 1962) and electromagnetic systems (Newman and Friedlander, 1950) have been among the systems used. Since the majority of studies on finger movement have been concerned with documentation of the exact morphology of patterns of normal and abnormal movement, transducers have been favoured which introduce the least distortion of movement patterns, and photoelectric systems have often been used for this reason (Cooper, Halliday and Redfearn, 1957). Our central concern in the selection of a transducer system was the capability of studying the effects of different forms of visual feedback displays on movement patterns obtained in a tracking task. Although we were concerned about analogue representation of changes in patterns of finger movement due to changes in sensory feedback, there was no reason to attempt representation of patterns of finger movement unaltered by physical properties of a transducer system. The transducer system which we have developed is shown in Figure 1. The index finger is extended, and inserted into an aluminium cup attached to a pulley system. A ball-and-socket joint permits the vertical component of any movement to be translated into motion of the pulley. The transducer is a carbon-film potentiometer attached to the lower shaft of the pulley system. The potentiometer provides an electrical signal proportional to the amount and direction of vertical excursion from a reference point.

FIGURE 1

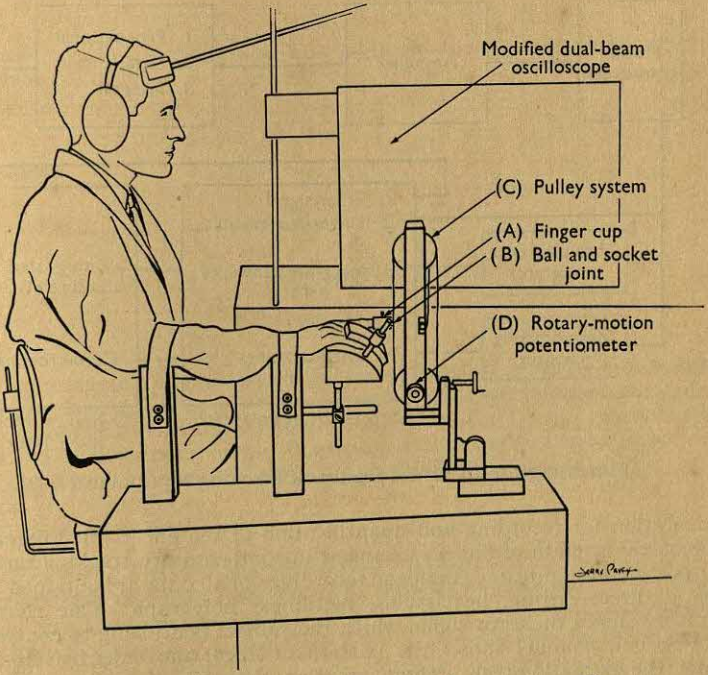

Modified dual-beam oscilloscope
(C) Pulley system
(A) Finger cup
(B) Ball and socket joint
(D) Rotary-motion potentiometer

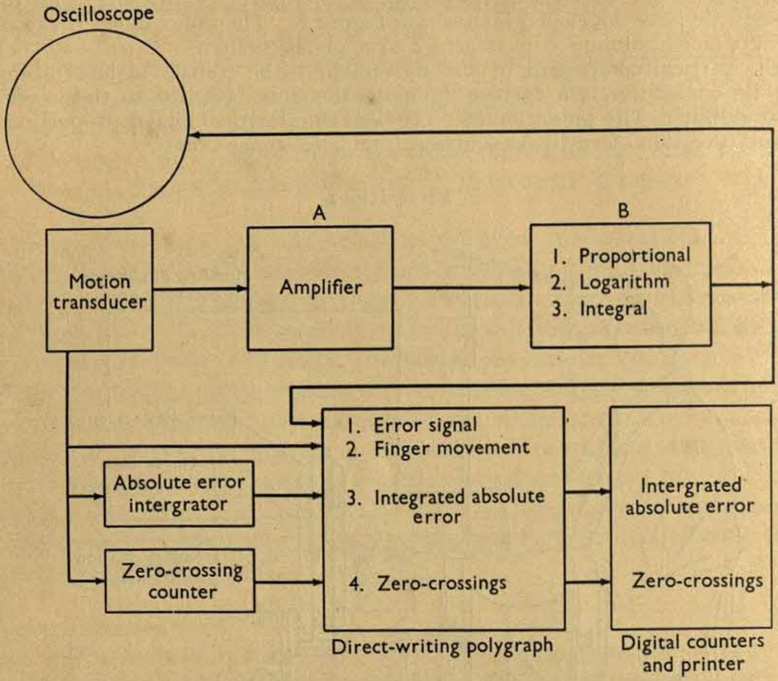
Diagram of visual feedback tracking equipment.

An accessory pulley permits weight-loading of the main pulley system. When weights are placed on this accessory pulley system, the subject must counteract the resultant force in order to maintain his finger at a fixed point in space. The relative amounts of flexion and extension activity required to maintain the finger at a fixed position in space can be altered by changing the direction in which the weights are placed over the accessory pulley. The relative amounts of extension and flexion activity can also be altered by changing the vertical position of the shaft containing the main pulley system, and thus establishing a different reference position in space at which the subject must try to hold his finger.

The subject performs a compensatory tracking task in which he is required to correct the movement of his finger from a fixed position in space using error information presented by means of visual or auditory displays.

In the simplest case, the error-signal will move in a manner directly proportional to the movement of the finger in space. However, it is possible to present the error-signal in a variety of other forms. These capabilities are shown in the box labelled "B" in the flow-diagram of equipment shown in Figure 2. We can shape the output of the potentiometer to present the logarithm (base 2), integral or derivative of the absolute voltage obtained. The gain of the error signal can be varied independently of the form of the signal being displayed. This is accomplished by adjusting the gain of an operational amplifier (box "A" in Fig. 2).

FIGURE 2

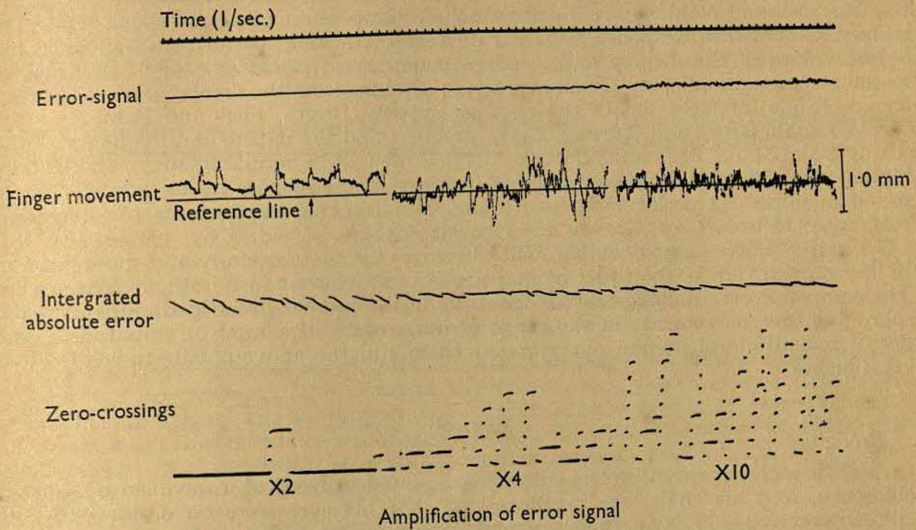


Flow diagram of visual feedback tracking equipment.

Instrumentation for recording and quantification of the effects of different forms of error signals on the pattern of finger movement in compensatory tracking tasks is shown in the lower portion of Figure 2. Analogue recordings of all data are obtained by using an Offner type R direct-writing, heat-stylus, rectilinear polygraph. One channel of the polygraph record shows the error signal which the subject is utilizing to control his finger position. A second channel shows the pattern of finger movement as it is obtained directly from the potentiometer. There are two channels of on-line data reduction. The first is the output of an integrator circuit which integrates the area traced out by the subject's finger as it moves above and below the reference position. Since the total area defined by the finger movement over a given period of time serves as a measure of the degree to which the finger was off target during that time, we refer to this measure as the absolute error. The second data reduction channel shows a cumulative count of the number of times the finger passes through the reference position. A sample-period clock establishes the period of time over which the zero-crossing and integrated absolute error scores are determined. This clock can be set at any value from 1 to 16 sec. At the end of the sample period, the data reduction channel pens reset to their baselines, and begin a new period of scoring.

Figure 3 shows a composite polygraph record for a subject using a proportional visual error signal with display gains of 2, 4 and 10. Channel 1 shows the error signal which the subject was utilizing to control his finger movement. The actual pattern of finger movement corresponding to the utilization of a proportional error signal is shown on channel 2. Channel 3 shows the output of the absolute error integrator. The vertical displacement of each of the diagonal lines on this channel is directly proportional to the area traced out by the finger about the reference position for each 3 sec. period. Each dot or bar on channel 4 corresponds to one unidirectional passing of the finger through the reference position. The integrated absolute error and zero crossing scores which are shown in analogue form in the polygraph record are simultaneously recorded in digital form using Hewlett-Packard AC-4 decade counters and type 560-A digital recorder (Fig. 2).

FIGURE 3



Polygraph record.

The above discussion characterizes the broad outlines of an instrumentation system which has been designed to study formal interrelationships between patterns of sensory feedback and patterns of motor activity in normal subjects and patients with abnormal movements. The remainder of the discussion will consist of specific examples of the manner in which such interrelationships are established experimentally.

C. *Objectives of the present study*

It is the primary purpose of the present study to examine the effect of display gain on the pattern of finger movement in a compensatory tracking task utilizing a proportional visual feedback display. In an effort to define some generalizations about the effects of increasing the gain of sensory feedback information on the response characteristics of human motor control operations, we shall cite some preliminary data showing the effects of increasing the gain of integral and logarithmic forms of visual feedback display on patterns of finger movement, and data obtained from experiments in which the gain of an auditory feedback signal was varied instead of a visual feedback signal.

D. *Literature survey on display gain in tracking tasks*

A great amount of investigation has been done concerning the effects of response and display magnitudes on tracking performance (Andreas and Weiss, 1954). Most of the literature in this area has been undertaken within the broad field of human engineering, with concerns about specific man-machine relationships. The result is a complex literature demonstrating little uniformity with respect to the representation and control of experimental variables, and a correspondingly complex and non-uniform nomenclature. Problems of nomenclature as they pertain to studies of output/input amplitude relations in man-machine systems have been recently discussed by Gibbs, who suggests the term "gain" to denote the display/control (or response) amplitude ratio (Gibbs, 1963).

Early studies concerned the relationships between response and display amplitudes for knob and lever-controlled tracking tasks (Craik and Vince, 1945; reprinted, Craik and Vince, 1963; Jenkins and Olson, 1952; Jenkins and Karr, 1954). The studies show that gain constitutes the most critical factor in determining the accuracy of control movements.

The literature on response display ratios as they affect pursuit and compensatory tracking performance demonstrates that the optimal response/display ratio will vary sensitively as a function of other system variables (Obermayer, Swartz and Muckler, 1962; Fitts, Marlowe and Noble, 1953; Senders, 1959). Important variables which affect the relationships between response/display ratios and tracking performance include the absolute values of the display and response magnitudes (Rockway, 1962), time delays between response and display (Rockway, 1954), distance from the display (Shackel, 1954), force-amplitude interrelations of the response system (Briggs, Fitts and Bahrick, 1957; Andreas, Gerall, Green and Murphy, 1957), practice, and the nature and frequency of the tracking task (Noble, Fitts and Marlowe, 1953; Bowen and Chernikoff, 1957), and display sampling rate (Bennett, 1956). Increases in the response display ratios, however, have resulted in improved performance in a number of tracking experiments (Andreas and Weiss, 1954; Helson, 1949; Garvey and Henson, 1958).

The major source of non-comparability between the present study and those cited in this literature review is the order of magnitude of movement in question. The nature of the compensatory tracking task under study in the present investigation involves the display of finger movements in the range of hundredths of a mm. in amplitude. The order of magnitude of human operator movements in the above-noted studies is often several inches.

METHOD

A. Procedure

1. *Principal experiment.* The subject was seated in front of a modified dual-beam oscilloscope, and his chair was positioned so that his eyes were on a line with the oscilloscope screen and 12 to 18 in. from the screen. The dominant forearm, wrist and hand rested on a hand rest with the index finger extended parallel to the ground. The distal phalanx of the index finger was secured in an aluminium cup attached to the motion transducer system (Fig. 1, A). The vertical shaft of the motion transducer system was then adjusted so that zero voltage was obtained from the rotary motion potentiometer attached to the lower shaft of the pulley system. When this mechanical adjustment was made, there was superimposition of the error signal beam upon the reference beam (both presented as 1 mm. wide, 2 cm. long horizontal bars) on a 5 in. diameter oscilloscope screen. In this manner electrical and mechanical reference positions were brought into alignment. After alignment, the error signal beam on the oscilloscope screen served to inform the subject about the position of his index finger relative to the position in space at which it would be if extended parallel to the ground. A proportional display of the voltage obtained from the potentiometer was used in the main experiment to be reported, and the error signal therefore moved above and below the fixed reference signal on the oscilloscope screen in direct analogy with the movement of the extended index finger above and below the reference position in space.

The principal experiment was designed to study the effects of gains of 1, 2, 4, 10, 20 and 40 in the presentation of a proportional error signal on the patterns of movement obtained in a continuous compensatory tracking operation. This distribution of values was selected because it was anticipated that small changes in gain from 1 to 10 would probably have large effects on performance (Garvey and Henson, 1958), and that the motor control system would be stable with respect to large changes in gain beyond the point at which appreciable improvement in motor control had been effected (McRuer and Krendel, 1957). The exact values were selected because of ease of appropriate circuit design. These six display conditions were presented to the subjects in random order. The subject was told that one of the lines on the oscilloscope screen would remain fixed in the mid-vertical position, and that the other would move above and below the fixed reference beam as a function of the movement of his finger above and below the reference position in space. The subject was asked to slowly move his finger up and down under the unity gain condition so that these relationships were clear to him before the experiment began. Before each condition the subject was told to look at the screen and to bring the two lines on the screen together and to hold them together as closely as possible until told to rest, at which time the subject was told to let his index finger fall to a rest position of flexion, and to look away from the screen. Each test condition lasted 140 sec. The subject was permitted to rest for 60 sec. between each of the six test conditions. The background illumination in the test room was maintained at a constant low level by covering windows with opaque shades and utilizing two artificial light sources to permit viewing of equipment controls, but carefully placed in order to avoid glare on the oscilloscope screen. Some complex auditory signals were generated by relays and other electro-mechanical components of the instrumentation system, and no measures were taken to eliminate these sensory inputs to the subject. We have subsequently gained considerable experience with this experiment performed under conditions in which the subject wears headphone enclosures which provide effective attenuation of acoustic input from the instrumentation system, and the results do not appear to differ from those obtained without using headphone enclosures.

2. *Dominant and non-dominant hand performance.* Ten of the 25 subjects used in the principal experiment were also tested using the non-dominant hand.

3. *The effects of high-gain proportional error signals on tracking performance.* A preliminary review of the data obtained in the principal experiment showed that most subjects maintained stable movement patterns even under the highest error signal gains studied. In order to obtain additional information about the gain stability limits of the motor control operations under study, we examined the movement patterns obtained from 10 subjects with visually-presented proportional error signals of gains 5, 10, 20, 50, 100 and 200. The gain conditions were presented in order of ascending value.

4. *The effects of increasing the gain of visually-presented non-proportional error signals on tracking performance.* The principal experiment was repeated under conditions in which the error signal was not proportional. Integral and logarithmic (base 2) transforms of the error signal were studied separately, and data was also obtained using a proportional signal alone. A proportional error signal was first presented using gains of 2, 5 and 10 in that order. The logarithmic error signal was then studied at three gains, presented in increasing order.

Four time scale factors for the integral error signal were studied: 2, 10, 20 and 100 sec.$^{-1}$.

This experiment was undertaken in order to observe whether there are general changes in the pattern of motor response which are due to increasing gain of a visually-presented error signal, independent of the basic form of the error signal.

5. *The effect of increasing the gain of an acoustically-presented error signal on performance in the compensatory tracking task.* The acoustic tracking equipment consisted of two amplitude-modulated, square wave tones of 1,400 and 1,000 cps. The 1,400 cps signal was utilized to provide indication of the magnitude of finger displacement above the reference point in space. The 1,000 cps tone provided similar information of displacements below the reference point in space. The reference point was indicated by absence of the two tones and a low level non-specific noise signal.

The relationship between finger displacement and tone amplitude was changed by means of the transducer amplifier gain settings. Visual and auditory gains were not equated on either physical or psychophysical scales.

This experiment was undertaken in order to observe whether there are general changes in the pattern of motor response which are due to increasing the gain of an error signal independent of the sensory channel used for the error signal presentation.

B. *Subjects*

The subjects for these experiments consisted of male and female volunteers in good general health in the age range 18–55. The majority of the subjects were in the young-adult age range.

RESULTS

Table I shows the results of the principal experiment in terms of the two aspects of performance scored: zero-crossings (Z.C.) and integrated absolute error (Ab.E). For both of the performance variables, the scores in Table I are averages based on twelve consecutive 10-sec. samples for each subject in each condition. The Ab.E. score unit was selected so that an Ab.E. score of 124 corresponds to an average deviation of 1 mm. from the reference point in space over a 10-sec. period.

TABLE I

INDIVIDUAL ABSOLUTE ERROR AND ZERO-CROSSINGS PERFORMANCE MEASURES AT SIX DIFFERENT AMPLIFICATIONS OF A PROPORTIONAL ERROR SIGNAL, FOR TWENTY-FOUR NORMAL SUBJECTS USING THE DOMINANT HAND, GROUPED BY SEX AND ORDERED ON AGE

Subjects		*Absolute error scores*						*Zero-crossings scores*					
		Error signal amplification						*Error signal amplification*					
No.	*Age*	*1*	*2*	*4*	*10*	*20*	*40*	*1*	*2*	*4*	*10*	*20*	*40*
	Male												
1	18	34	14	11	7	7	10	7	6	18	12	8	16
2	19	94	48	24	36	30	35	12	26	16	23	22	24
3	19	28	19	14	10	11	11	2	6	9	13	14	11
4	20	21	13	11	9	12	9	6	19	22	22	24	25
5	22	38	19	13	8	8	8	2	8	28	19	24	20
6	22	56	32	21	11	11	17	1	0	18	21	21	25
7	22	12	10	9	9	7	8	9	21	22	20	32	27
8	23	20	16	7	6	5	5	3	3	8	14	19	17
9	23	20	9	10	9	10	8	6	9	8	8	16	8
10	25	24	13	11	9	12	8	3	17	27	24	18	23
11	51	83	38	25	13	8	10	0	2	1	6	14	11
12	55	93	27	22	14	10	9	0	3	7	6	14	12
	Female												
13	18	36	14	11	9	7	8	2	7	8	11	11	11
14	18	24	13	12	12	11	9	5	14	14	13	15	14
15	19	13	16	7	7	6	5	5	2	7	10	6	9
16	20	30	13	8	5	8	7	1	3	18	29	38	31
17	23	15	14	7	6	7	7	7	5	14	13	14	21
18	25	15	8	6	7	6	6	6	13	11	13	15	13
19	30	24	12	7	9	33	13	2	9	21	17	13	17
20	32	40	34	48	10	10	14	11	27	21	19	27	36
21	38	13	11	8	6	7	9	10	23	31	35	34	48
22	44	27	62	41	17	9	11	6	6	13	21	17	22
23	54	44	15	11	13	9	9	7	15	17	36	37	42
24	54	45	15	10	7	7	7	0	4	6	9	13	15
Median* ..		26	16	11	10	9	10	5	8	14	14	17	18
Mean* ..		35	20	15	10	10	10	5	10	15	17	19	21
S.D.* ..		24·0	13·4	10·7	6·2	6·8	6·0	3·6	8·1	7·8	8·2	8·8	10·4

* Based on the total group, N = 24.

Table II presents comparable performance scores for the same task performed with the non-dominant hand for ten of the subjects. The subjects' numbers in Table II correspond to their identity in Table I.

The trend of the discrepancies between group means and corresponding medians incidates the variables have skewed distributions. Inspection of the standard deviations reveals an obvious correlation between mean and variance. Because the data have these characteristics, non-parametric tests were used for the majority of the statistical comparisons. The data in Table I were ranked, and Friedman's analysis of variance for ranked data was employed to test the overall effects of the two main variables: subjects and gains (Siegel, 1956). For the Ab.E. scores, the evidence for individual differences was found to be significant at the 0·005 level, and the effect of gain significant at the 0·005 level. For the Z.C. scores, individual differences were found to be significant at 0·005, and the gain effect significant at 0·005.

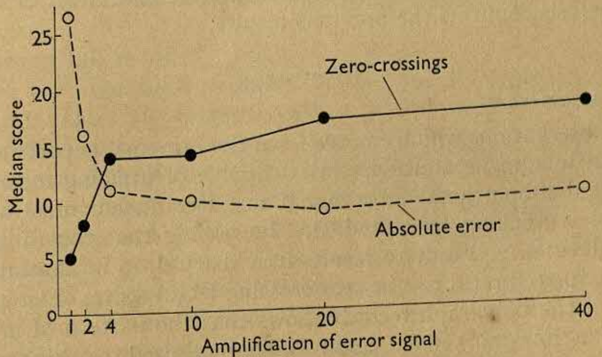
TABLE II

INDIVIDUAL ABSOLUTE ERROR AND ZERO-CROSSINGS PERFORMANCE MEASURES AT SIX DIFFERENT AMPLIFICATIONS OF A PROPORTIONAL SIGNAL FOR TEN NORMAL SUBJECTS USING THE NON-DOMINANT HAND

	Absolute error scores						*Zero-crossings scores*					
	Error signal amplification						*Error signal amplification*					
Subjects	*1*	*2*	*4*	*10*	*20*	*40*	*1*	*2*	*4*	*10*	*20*	*40*
2	61	44	16	27	20	18	5	5	13	10	13	20
4	22	30	13	11	13	12	16	20	37	29	32	31
8	34	15	10	8	10	6	2	10	12	23	16	22
10	20	15	12	10	10	9	4	16	17	24	16	23
11	102	36	18	27	10	11	2	1	5	12	25	15
14	14	13	8	7	8	8	9	9	12	12	11	12
15	10	9	10	4	6	6	3	4	3	8	7	5
20	33	16	12	7	9	24	15	6	18	10	30	42
22	36	48	35	13	11	11	5	3	6	8	18	18
24	22	12	10	7	7	7	6	5	7	15	22	20
Median ..	28	16	12	9	10	10	5	6	12	12	17	20
Mean ..	35	24	14	12	10	11	7	8	13	15	19	21
S.D. ..	27·5	14·4	7·8	8·2	3·9	5·8	5·1	6·0	9·8	7·5	8·2	10·2

The pattern of the change in performance with increasing gain is apparent in Figure 4 which shows the median score for Ab.E. and Z.C. at each of the gains tested. To estimate the statistical significance of the apparent differences between specific gains, and to make a coarse analysis of the trend in the data, the full set of multiple comparisons were made comparing the result at each gain level to the results at every other gain. Wilcoxon's test for matched-pairs (Siegel, 1956) was applied as a test of the statistical significance for each comparison. The results of the fifteen comparisons for each performance variable are presented in matrix form in Table III.

FIGURE 4



Group medians (N = 24) of zero crossing and absolute error scores for the proportional amplification experiment.

TABLE III

RESULTS OF SIGNIFICANCE TESTS OF DIFFERENCES IN SCORES BETWEEN GAINS, TABLE OF P-VALUES*, TWO-TAILED TESTS, N=24 PAIRS IN EACH COMPARISON

		Ab.E. Scores				
		Gain				
		2	*4*	*10*	*20*	*40*
Gain	*1*	0·01	0·01	0·01	0·01	0·01
	2		0·01	0·01	0·01	0·01
	4			0·01	0·01	0·01
	10				N.S.	N.S.
	20					N.S.
		Z.C. Scores				
		Gain				
		2	*4*	*10*	*20*	*40*
Gain	*1*	0·01	0·01	0·01	0·01	0·01
	2		0·01	0·01	0·01	0·01
	4			N.S.	0·02	0·01
	10				0·05	0·01
	20					N.S.

* Table entry N.S. indicates difference not significant at 0·05.

For the dominant—non-dominant comparison, Wilcoxon's test was applied to the scores totalled across conditions. Test results showed that difference in performance between the two hands, if any, fails to achieve statistical significance for either Ab.E. or Z.C. scores.

Transforming the Ab.E. scores in Table I to natural logarithms appeared to remedy the skewness and heterogeneity of variance in that distribution. A simple conversion of the gain values results in an approximately linear scale, X′, for the gain variable: $X' = 3{\cdot}195 \log_{10} X$ where X represents any of the gain values used in this experiment.

Making both these transforms yielded a set of data suitable for a parametric analysis of variance (Boneau, 1960, 1962) and a more exact test of the trend in the data. This analysis confirms the previous results.

DISCUSSION

A. Principal experiment

The major observations which emerge from the principal experiment demonstrate that the normal human nervous system is capable of utilizing increasing gain of a visually-presented proportional error signal to effect finer control of movement in the compensatory tracking task studied. Increasing the gain of the error signal through the values: 1, 2, 4 and 10 results in a sharp drop in absolute error (Ab.E.) scores and a sharp reciprocal rise in zero crossing (Z.C.) scores (Fig. 4).

Inspection of the polygraph records shows that the pattern of movement which corresponds to a low-gain error signal is characterized by high amplitude, low frequency activity (Fig. 3). As the gain of the error signal increases, there is progressive decrease in the amplitude of the low frequency movement components, and progressive increase in the amplitude of high frequency movement components. It thus appears that the progressive improvement of motor control in tracking performance which results from increasing the gain of the error signal consists of the progressive appearance of high frequency activity of low amplitude, precisely centred about the reference position in space. Although there are marked individual differences with respect to the quantitative aspects of these changes in motor response patterns, the qualitative relationships are characteristically observed.

It might be argued that the high amplitude, low frequency activity that characterizes performance under low gain conditions is largely due to inadequate separation of the two lines of the visual display. It is true that a subject who has little involuntary movement when his finger is in full extension parallel to the ground will frequently remain away from the reference position in space for a considerable time before his finger moves a sufficient distance to result in clear-cut separation of the error signal beam from the reference beam under a low gain condition, and thereby effect a very large absolute error. However, we have examined many normal subjects who have considerable involuntary movement when they extend their finger, and thus effect frequent, obvious separation of the error signal from the reference beam, even under low gain conditions; and in these cases also we observe a prominent high amplitude, low frequency movement component under low gain error signal conditions. We have even observed this to be the case in patients with abnormal tremor activity which is fifty times or greater in amplitude than the mean amplitude of most normal subjects' movement patterns. It follows that the high amplitude, low frequency movement patterns which appear when a low gain error signal is being used for monitoring in a compensatory tracking task is not simply due to inadequate separation of the two lines of the visual display.

It is possible that there is a range of display amplitudes below which there is no initiation of error-correcting responses. It is clear that sufficient separation of the error signal from the reference beam results in the initiation of a voluntary corrective response. The above hypothesis suggests that there is an amplitude threshold for the initiation of corrective responses. If this hypothesis is correct, it would explain the high amplitude, low frequency movement pattern seen under low gain conditions; for most of the displacements of the error signal beam from the reference beam under these conditions might be below the threshold for initiation of corrective responses. The same hypothesis would explain the progressive increase in the dominant energy frequency as a function of increasing gain of the error signal; for increasing gain of the error signal would result in progressively more displacements of the error signal from the reference beam of sufficient amplitude to result in the initiation of a corrective response.

There is a sharp fall in Ab.E. scores as a function of increasing gain over the range 1, 2, 4 and 10. The Ab.E. scores under the high gain conditions do not differ significantly (Table III). The reciprocal sharp rise in Z.C. scores as a function of the gain of the error signal is also restricted to the gain range: 1, 2, 4 and 10. The Z.C. scores under high gain conditions plateau, and do not show the consistent significant differences which are found when Z.C. scores under low gain conditions are compared (Table III). Although most of the advantage of increasing gain with respect to improvement in tracking performance has been obtained by the time the X10 gain condition is reached, the motor control system maintains a stable pattern of movement despite a fourfold increase in gain beyond this point.

There must be some minimum amount of movement over which no voluntary control can be exercised. It should be recalled that the subject is required to maintain his finger extended parallel to the ground. This requires active maintenance of neuromuscular activity and would be expected to generate background movement upon which control might be superimposed, but which would remain as the unmodifiable expression of this neuromuscular activity. For many normal subjects, the mean amplitude of movements of the extended index finger are measured in hundredths of a mm. However, even in those cases in which there is extremely little movement of the extended finger, the pattern of movement is quite different from that obtained when the extremity is at rest. In the latter case, a ballistic component due to the peripheral pulse makes the predominant contribution to the movement pattern (Brumlik, 1962).

Although a limit of movement beyond which further increase in the gain of the error signal will not result in additional control is entirely expected, it remains to explain the stability of this limit of motor control as the gain is increased markedly beyond the point which affords maximum control. Seven out of the ten subjects who performed tracking tasks with proportional visual feedback signals at gains of 5, 10, 20, 50, 100 and 200 maintained excellent control of their finger movement under the highest gain conditions. These subjects' polygraph records sometimes showed progressive increase in the dominant energy frequency under high gain conditions, however absolute error scores did not differ markedly under any of the gain conditions. This observation indicates that the human motor control system, as studied in our tracking experiments, has considerable stability, and suggests the possibility of self-optimizing system capabilities (McRuer and Krendel, 1957).

There is apparently some learning involved in the capability of maintaining stable motor control under high gain conditions, for we occasionally observed the appearance of oscillating high amplitude, high frequency activity when a subject was required to utilize a high gain error signal early in the random presentation of the six gain conditions. Whenever this occurred, we could invariably repeat the high gain condition at the end of the series of conditions without producing the oscillating activity.

Most subjects showed occasional spontaneous high-amplitude movements under all gain conditions. Such movements were more frequent under high gain conditions, and whereas they were characteristically rapidly corrected under low-gain conditions, they often resulted in brief periods of sustained oscillations under high-gain conditions. This observation suggests that although the normal subject is apparently capable of maintaining extremely stable motor control even under high-gain proportional error signals, the high-gain error signal conditions nonetheless predispose to instability if a relatively large displacement of the finger should occur (Rockway, 1957).

Most of our discussion has concerned the relationships between the display gain of a visually-presented proportional error signal and corresponding patterns of tracking movement, independent of consideration about the other channels of sensory feedback accompanying the movements under study. It should be noted however that there is a complex pattern of sensory feedback generated by the type of task we have had our subjects perform. This feedback pattern has contributions from the skin receptors in the finger (Provins, 1955), the muscle and joint receptors in the forearm (Barker, 1962), and the receptors which are activated by movements of the extra-ocular muscles (Sampson, Coleman and Elkin, 1959). None of these sensory feedback channels were studied in the experiments reported above.

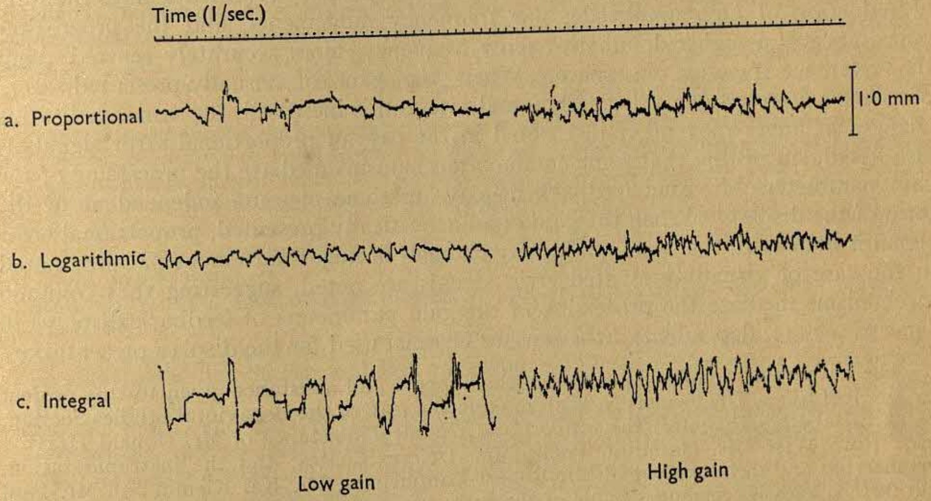
B. *Non-dominant hand performance experiment*

The major findings reported in the above discussion of the principal experiment were also observed when the non-dominant hand was tested. These findings suggest that the relationships observed between display gain and motor response patterns reveal basic mechanisms of motor control which function in independence of learning which might be expected to bias control in favour of the dominant hand.

C. *Experiment on increasing the gain of non-proportional error signals*

The use of proportional and logarithmic error signal displays result in characteristic differences in finger movement patterns. These differences in movement patterns preclude the use of Ab.E. and Z.C. scores as valid measures for comparison of the effects of gain on movement patterns as the form of the error signal is varied. However, it was possible by inspection to discern a common feature in the gross morphology of finger movement patterns as the gain of the error signal display was increased, independent of the form of the error signal. High amplitude, low frequency movements were noted to characterize performance under low-gain conditions, and higher dominant energy frequencies were often noted to characterize performance under high gain conditions (Fig. 5).

FIGURE 5



Polygraph records obtained from one subject showing the effects of low and high gain visual feedback displays on finger movement patterns when different forms of error signals are being utilized for tracking.

The above observations suggest that increasing the gain of visually-presented error signals effects patterns of finger movement in a characteristic manner, which is in part independent of the basic form in which the error signal is presented.

D. *Experiment on increasing the gain of an acoustically-presented error signal*

The effects of increasing the gain of visually-presented proportional error signals on finger movement tracking patterns were compared with the effects of increasing the gain of acoustically-presented proportional error signals for the same tracking task. It was noted that low gain signals resulted in high amplitude, low frequency movements, usually far removed from the reference position, independent of the sensory channel used for the error signal presentation. As the gain of the error signal was increased, a progressively higher dominant energy frequency appeared, which was progressively more accurately centred about the reference position in space. These effects were also independent of the sensory channel utilized for error signal presentation. The comparisons of the effects of visual and auditory error signals on patterns of finger movement tracking were qualitative, and based exclusively upon inspection of the polygraph records. Since no psychophysical techniques were utilized to equate gain conditions for the visual and auditory systems, it was considered inappropriate to make a fine quantitative comparison of absolute error and zero crossing scores as a means of comparing the effects of error signal presentation to different sensory channels. The fact that increasing gain of proportional error signals resulted in qualitatively similar patterns of finger movement tracking for both visual and auditory display conditions suggests that some common mechanisms mediate the processing of the gain parameters of sensory feedback in some measure independent of the sensory channel utilized for feedback presentation.

Conclusions

Increasing the gain of visually-presented, proportional error signals results in marked improvement in the ability of subjects to maintain an extended index finger at a fixed point in space. The improved control of finger movements takes the form of reduction of high amplitude, low frequency movements, and the progressive appearance of a higher dominant energy frequency, more accurately centred about the reference position in space. When the gain of visually-presented, non-proportional error signals (logarithm, and integral) is increased, the same qualitative changes in finger movement just noted in the case of proportional error signals is also noted, suggesting that some common mechanisms mediate the processing of the gain parameters of visual feedback displays in some measure independent of the form of the display. When the gain of an acoustically-presented, proportional error signal is increased, the same qualitative changes in finger movement patterns noted in the case of visually-presented error signals are noted, suggesting that common mechanisms mediate the processing of the gain parameters of feedback displays in some measure independent of the sensory channel used for the display presentation.

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THE LIMITS OF OVER-CONSTANCY

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A number of studies have led to the generalization that there is a tendency to "over-constancy" in the perception of size. The experiments reported here suggest that over-estimation is limited to objects subtending angles of approximately 2° or less, and that objects subtending greater angles are judged fairly accurately. This raises the possibility that foveal diameter (approximately 2°) is involved.

INTRODUCTION

In a recent review of size-distance experiments, Epstein, Park and Casey (1961) state: "A frequently confirmed finding is size-overestimation which increases with distance. As the physical distance of the object is increased, the physical size of the object is progressively over-estimated." They refer to the experiments of Holway and Boring (1941), Gibson (1947, 1950), Smith (1953), Gilinsky (1955), and Jenkin (1957, 1959); which together cover the range of distances from 2 ft. (Jenkin) to 4,000 ft. (Gilinsky). Denis-Prinzhorn (1960) gives further evidence and an excellent review. Epstein *et al.* seem to consider that these experimental results demonstrate a general characteristic of normal perception; thus they conclude that "increasing over-estimation of size is well-established." It should be emphasized that this generalization refers to judgements of true or physical size.

No doubt many factors contribute to these results. Carlson (1960, 1962) suggests attitude. A tendency to over-estimate a standard object, found by Piaget and Lambercier (1943) may also contribute, since in these experiments the standard is usually the more distant object. This paper draws attention to a further possibility. We began by asking how far the results might be dependent upon the size of the target.

The majority of the experiments use targets of less than 6 ft. Holway and Boring use standards subtending an angle of 1 degree. Smith, and also Jenkin, use standards of 4 in. or less. The largest size for which Gilinsky was able to report reliable results was 66 in. The only experimenter to use standards greater than 6 ft. was Gibson. His targets ranged from 15 to 99 in., with a mean height of 69 in. But it is noticeable that the over-constancy in Gibson's results is of the order of only 5 per cent., and that he himself concludes that "an object can apparently be seen with approximately its true size as long as it can be seen at all" (1950). Further, in Gilinsky's results, the degree of over-constancy decreases with increase in the size of the target. The average degree of over-estimation, for distances from 200 ft. to 4,000 ft. is, for a 42-in. standard approximately 30 per cent., for a 54-in. standard approximately 20 per cent., and for a 66-in. standard approximately 10 per cent. (writers' calculation).

These observations suggested that over-estimation might disappear if larger targets were used. We also thought it possible that, if a sufficient range of sizes was examined, a central tendency effect might appear, with under-estimation at the upper end of the scale. The first two experiments provided some encouragement for this hypothesis. The second two experiments, however, made it less plausible. Finally, it became necessary to reconsider all the data. It seems now that the crucial factor is not the physical size of the target, but the angle subtended by the target. The following experiments suggest that objects subtending angles of approximately 2° or more are judged fairly accurately, but that as the angle falls below 2° the object is progressively over-estimated. In the experiments referred to by Epstein *et al.* almost all targets subtended angles of less than 2° (present writers' calculations), and the increasing over-estimation with distance to which they refer may be a consequence of the diminishing angle subtended by the object in question.

Experiment I

(*a*) *Method*

Environment. The experiment was conducted on an airfield which offered a level stretch of grass about 600 yd. long. Beyond this distance the ground dropped, so that the subject was presented with an empty horizon. No objects were visible within 30° on either side of the target.

Apparatus. The apparatus consisted of five rectangular planks of white-painted wood, with lengths of 12, 24, 48, 84 and 132 in., and widths of 3, 3, 4, 5 and 9 in. respectively. These were held vertical by a device which was invisible to the subject. Each was presented singly at distances of 27½, 110, and 440 yd., making 15 conditions in all.

Subjects. Ten independent subjects were examined singly under each condition, making 150 subjects in all. The subjects were Air Training Corps cadets, all male with ages ranging from 13 to 18 years. These subjects had no previous experience of perceptual experiments.

Procedure. Each subject was first asked "How tall do you think that white object is?" He was then asked: "How far do you think it is?" No practice judgements were given. Each subject simply made one judgement of size and one of distance. Most subjects spontaneously gave their answer in terms of feet and yards. Where necessary, they were prompted to do this.

It will be seen that we asked the subject for an *absolute* judgement of a single object, whereas in previous experiments subjects have usually made *comparative* judgements of two or more objects. Our main reason is that the comparative method inevitably gives the subject some hint as to the range of sizes within which the experimenter expects his judgement to fall, and may restrict the subject to a certain range. We also consider that the absolute judgement may be a more natural one for the subject to make, and, by asking him for only one judgement, we suggest that our results approximate more closely to the kind of spot impression which one forms in everyday conditions. In the second experiment, we compare the absolute and comparative methods.

(*b*) *Results*

The following values were calculated for judgements of both size and distance: (1) means; (2) percentage errors; (3) standard deviations, and (4) standard errors. In the absence of homogeneity of variance, it was not possible to conduct an analysis of variance. The results however show a consistent pattern, and separate "t" tests have been applied to the discrepancies between actual and judged size, and distance, under each experimental condition.

Judgements of size

The following trends are shown by the data (see Table I).

(1) As might be expected, standard deviations increase with size (in eight of 12 cases), and with distance (in eight of 10 cases).

(2) Smaller targets (12 in. and 24 in.) are over-estimated in six of six cases, significantly in three cases. Larger targets (84 in. and 132 in.) are under-estimated in five of six cases, significantly in one case. The under-estimation of large targets is less marked and consistent than the over-estimation of small targets.

TABLE I

ABSOLUTE JUDGEMENTS OF SIZE

EXPERIMENT I

Measure	*Distance* (*yd.*)	*Size of target* (*in.*)				
		12	24	48	84	132
Mean ..	27·5	14·7	27·9	43·3	73·2	118·8
	110·0	17·4	29·3	50·4	80·9	129·6
	440·0	40·2	36·0	56·4	83·4	134·4
	Mean ..	24·1	31·1	50·0	79·2	127·6
Percentage Error ..	27·5	+22·5	+16·2	−9·8*	−12·9*	−10·0
	110·0	+45·0*	+22·1*	+5·0	−3·7	−1·8
	440·0	+235·0*	+50·0	+17·5	−0·7	+1·8
	Mean ..	+100·8	+29·4	+4·2	−5·8	−3·3
Standard deviation ..	27·5	4·03	5·67	5·37	6·82	26·04
	110·0	5·87	5·03	6·45	16·61	15·28
	440·0	15·24	19·80	14·41	17·98	54·72
	Mean ..	8·38	10·17	8·74	13·80	32·01
Standard error ..	27·5	1·34	1·89	1·79	2·27	8·68
	110·0	1·96	1·68	2·15	5·54	5·09
	440·0	5·08	6·60	4·80	5·99	18·24

* = significant at 0·05 or less, based upon "t" with 9 d.f.

(3) There is a consistent tendency for the estimation of the size of a target to increase with its distance (i.e. more distant targets are over-estimated in comparison with nearer targets).

Judgements of distance

In all our experiments judgements of distance have shown great variability, so we omit them. A tendency to under-estimate distance is possibly present, but there is no useful evidence on the relationship between size and distance judgements, except possibly the negative evidence that the marked tendency to over-estimate the size of the small targets is not accompanied by a tendency to over-estimate their distance.

EXPERIMENT II

(*a*) *Method*

Environment. The experiment was conducted on a level field of mown grass. The chief difference between this environment and that of the first experiment, was that this field was bounded on all sides by mature trees and shrubs, thus forming a clearly marked and relatively closed space, about 300 yd. long, whereas in the first experiment the horizon was open, and at a greater and less definite distance.

Apparatus. The standard objects were four rectangular planks of white-painted wood, with lengths of 12, 42·5, 87 and 168 in., and widths of 2·75, 3·5, 2·0 and 5·5 in. respectively. These were held vertical by a device which was invisible to the subject. Each was presented singly at distances of 27·5, 110 and 250 yd., making 12 conditions in all.

Subjects. Six independent subjects were employed under each condition, making 72 subjects in all. Fifty-one were members of H.M. Forces seconded to the university for a one-week course in statistics. The remaining 21 were university students. The subjects were thus predominantly young males of good intelligence and education. None had previous experience of experiments of this type.

Procedure. Each subject made four judgements.

(1) He was first asked to make an absolute estimate of the height of the target in feet and inches ("Absolute judgements").

(2) He was then asked to reproduce this estimate of absolute size, without further perception of the target. ("Reference absolute judgements.") This was done as follows: The experimenter held up a long rod, some 10 ft. from the subject and at right angles to the target. The tip of the rod was marked by a small white disc, and the subject was required to judge when the height of this small white disc above the ground equalled the estimate which he had just given of the height of the target. (A reel and line attached to the rod enabled the experimenter to measure the height of the disc quickly and accurately.)

(3) The subject was then instructed to compare directly the height of the white disc with the height of the target, glancing back and forth, and instructing the experimenter to raise or lower the disc until equality of true height was attained. ("Comparative judgements.")

(4) The subject was asked to estimate the distance of the target in yards. (No further reference will be made to these judgements.)

Judgement (1) corresponds to the judgement of size obtained in Experiment I. Judgement (2) was intended to provide some check on the reliability and validity of judgement (1). Judgement (3) was intended to indicate the relation between absolute and comparative judgements.

(*b*) *Results*

It was originally intended to apply analysis of variance to the results, but it is again clear that the basic condition of homogeneity of variance is not met. Furthermore, it is not possible to suggest any simple transformation function which would achieve this result (variance does not appear to alter systematically with the magnitude of the errors in judgement). But the results are highly consistent, and it is possible, as in the first experiment, to apply separate "t" tests to the discrepancies between actual and judged size under each experimental condition.

The results for the "absolute" judgments are given in Table II. Results for the two other types of size judgements are not given, but are available on request. The "reference absolute" judgements show the same pattern of positive and negative errors as the "absolute" judgements, with very comparable standard deviations and percentage errors. It seems therefore that subjects can translate their absolute judgement into a standard visible length with considerable consistency. The "comparative" judgements again show the same pattern of errors, and similar standard deviations, so that a fairly uniform pattern prevails throughout the judgements of size. The fact that these three judgements were always made in the same order, has no doubt contributed to this consistency, and thus limits the significance which can be attached to it.

The following trends are shown in the "absolute" judgements of size.

(1) As in Experiment I, standard deviations increase with size (in seven of nine cases), and with distance (in six of eight cases).

(2) Small targets (12 in.) are over-estimated in three of three cases, significantly in one case. Large targets (87 in. and 168 in.) are under-estimated in six of six cases, significantly in one case. As in Experiment I, the over-estimation of the smallest objects used is more marked than the under-estimation of the largest targets

used; but the under-estimation of the larger targets is more marked and consistent in Experiment II. (In the case of comparative judgements, small targets, 12 in., are over-estimated in three of three cases, significantly in all cases; while large targets, 87 in. and 168 in., are under-estimated in six of six cases, significantly in three cases.)

(3) The tendency for the estimation of the size of the target to increase with its distance, is shown in the case of the 12-in. target only.

TABLE II

ABSOLUTE JUDGEMENTS OF SIZE

EXPERIMENT II

Measure	*Distance* (*yd.*)	*Size of target* (*in.*) 12	42·5	87	168
Mean	27·5	15·2	38·2	79·5	143·0
	110·0	22·0	37·7	78·3	141·0
	250·0	26·5	45·7	69·8	138·0
	Mean ..	21·22	40·53	78·59	140·66
Percentage error ..	27·5	+26·4	−10·0	−8·6	−14·9
	110·0	+83·3	−11·4	−10·0	−16·1*
	250·0	+120·8*	+7·5	−19·7	−17·8
	Mean ..	+76·8	−4·6	−12·8	−16·3
Standard deviation ..	27·5	3·08	6·15	8·10	26·25
	110·0	8·94	4·57	10·73	21·28
	250·0	9·37	6·88	15·15	29·80
	Mean ..	7·13	5·87	11·33	25·78
Standard error ..	27·5	1·38	2·75	3·62	11·74
	110·0	4·00	2·04	4·80	9·52
	250·0	4·19	3·08	6·78	13·33

* = significant at 0·05 or less, based upon "t" with 5 d.f.

(*c*) *Discussion*

The extension of the upper range of size, as compared with Experiment I, has perhaps brought somewhat stronger evidence for the under-estimation of large targets; but the main finding is again that the small targets are over-estimated, and that this over-estimation disappears with increase in size. In the next experiment we extended the upper range of size still further. We also used circular instead of rectangular targets to determine how far the shape of the target is relevant.

EXPERIMENT III

(*a*) *Method*

The environment was the same as in Experiment II. The standard objects were four circles of white-painted wood, with diameters of 12, 48, 96 and 192 in. Each was presented singly at a distance of 110 yd. only, making four conditions in all. Twelve independent subjects were employed under each condition, making 48 subjects in all. The subjects were similar to those in Experiment II. The procedure was identical.

(*b*) *Results*

The results, again for absolute judgements only, are given in Table III. It will be seen that the smallest target, 12 in., is significantly over-estimated; but the remaining targets are not significantly different from the correct figure.

TABLE III

ABSOLUTE JUDGEMENTS OF SIZE

EXPERIMENT III (110 yards)

Measure	*Size of target* (*in.*)			
	12	48	96	192
Mean	18·3	55·4	98·6	190·0
Percentage error ..	+52·5*	+15·4	+0·8	−1·0
Standard deviation ..	5·9	15·5	13·1	49·4
Standard error	1·8	4·7	3·9	14·9

* = significant at 0·05 or less, based upon "t" with 11 d.f.

The results do not favour the central tendency hypothesis. The largest target, 192 in., is indeed under-estimated, but by a small and non-significant percentage.

EXPERIMENT IV

(*a*) *Method*

The experiment was conducted indoors, in a room 30 ft. by 20 ft. The standard objects were five circles of white cardboard, with diameters of 3, 15, 30, 48 and 96 in. Each was presented singly at distances of 6 and 20 ft., making ten conditions in all. Twenty independent subjects were employed under each condition, making 200 subjects in all. The circles were presented against a black background 10 ft. square. Each subject was asked to make one judgement only, namely, an absolute judgement of the diameter of the circle in feet and inches.

(*b*) *Results*

The results are given in Table IV. One target, that of 3 in. at 20 ft., shows a significant over-estimation; the remainder show no significant departure from the correct figure. Again, there is no support for the central tendency hypothesis.

DISCUSSION

A first negative conclusion, strongly indicated, is that there is no universal tendency to over-estimate size. Our four Tables cover 37 different combinations of size and distance, ranging from 3 to 192 in., and from 2 to 440 yd. In only six cases is there a statistically significant tendency to over-estimation.

A second negative conclusion, less strongly indicated, is that the hypothesis of a central tendency effect is not supported. Large objects were often under-estimated, but this was statistically significant in only three cases, and the effect is not very marked or consistent.

Taking the four experiments together, the most striking result is the apparent tendency for small objects at great distances (the 12-in. target at 250 and 440 yd.) to be grossly over-estimated. It occurred to us that this might be in some way connected with the extremely small angle which such targets subtend to the eye (4 and 2 min. of arc respectively). This suggested that the angle subtended by

TABLE IV

ABSOLUTE JUDGEMENTS OF SIZE

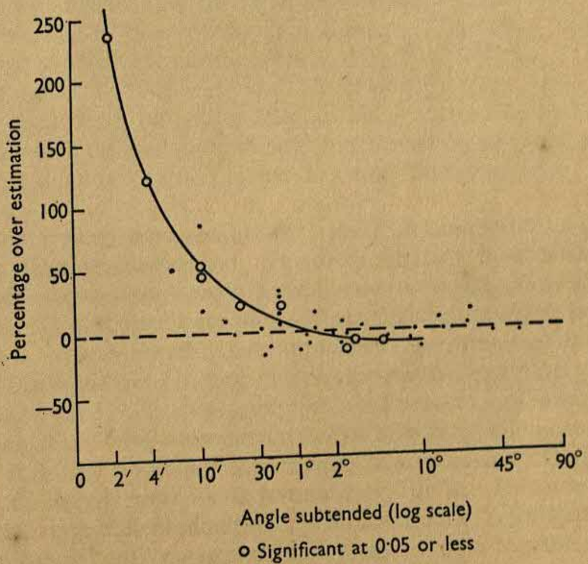
EXPERIMENT IV

		Size of target (in.)				
Measure	*Distance* (*ft.*)	3	15	30	48	96
Mean ..	6	3·2	16·3	34·5	46·9	93·8
	20	3·6	15·5	29·3	47·9	97·9
Percentage error ..	6	+6·6	+8·7	+15·0	−2·3	−2·3
	20	+20·0*	+3·3	−2·3	−0·2	+2·0
Standard deviation ..	6	0·57	3·66	6·26	9·79	15·3
	20	0·41	5·39	5·00	5·42	17·4
Standard error ..	6	0·13	0·83	1·45	2·24	3·51
	20	0·09	1·24	1·15	1·24	3·99

* = significant at 0·05 or less, based upon "t" with 19 d.f.

targets might be the crucial factor, so all the combinations of size and distance were re-expressed in terms of visual angle. The result is shown in Figure 1, where percentage over- or under-estimation is plotted against angle subtended. It is clear that if an object subtends an angle of less than 1° it is likely to be over-estimated, and that this over-estimation increases as the angle diminishes. Somewhere between 1 and 2° the over-estimation seems to disappear. At greater angles there may be a slight tendency to under-estimate, but on the whole objects are judged fairly accurately. The diameter of the fovea is usually given as approximately 1·5°

FIGURE 1



(Geldard, 1953) or 2° (Ruch and Fulton, 1960). Weymouth (1958) reviews spatial thresholds of the photopic eye which are functions of retinal eccentricity, but no close parallels seem evident. Our data show a relatively sudden change around 2°, whereas his show a more gradual alteration.

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BINOCULAR RIVALRY AND THE CONTRAST AT CONTOURS

BY

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Binocular rivalry was recorded between various achromatic figures in or near the foveae. For a pair of intersecting contours, one in the field of each eye, it was found that the percentage of time for which a contour was dominant at the point of intersection increased with the contrast at that contour, and also with average luminance when contrast was constant. Further, for 1° circles in corresponding positions in the two fields, one darker than its surround and one lighter, the same results were obtained. Various auxiliary results, on rate of rivalry, eye-dominance, the occasional mixture of the rivalling stimuli, and binocular lustre, are given. Finally the relationship between predominance in rivalry and perceived brightness is discussed.

INTRODUCTION

Binocular rivalry depends on the relative positions of the contours in the visual fields of the two eyes. This has often been observed, and can be seen by anyone who looks at suitable displays in a stereoscope. One might therefore expect the rate of rivalry and the predominance of each stimulus to depend on the contrast at those contours. The experiments to be described investigated this dependence.

Several authors have observed that if the luminance of one display, or of part of it, is greater than that of the other, it predominates in the rivalry; i.e. it is seen for more of the time than the other one. Breese (1899) recorded the rivalry of red and green squares and found that if the illumination of one was increased, it predominated. Precise observations were made by Fry (1936), who presented three vertical bars to one eye and three horizontal to the other so that the two sets formed a grille in the binocular field. As the luminance of the vertical bars, but not of the surrounds, was increased, they predominated more. Lawaetz (1939) found the same for the rivalry at the point of intersection of two slits of light. The importance of contrast was shown by Gellhorn and Kuckenburg (1924), who worked with pentagons of coloured paper on white, grey, or black surrounds, and found that in general the colour which differed more in brightness from its surround, predominated. The relation of predominance to contrast has not, however, been systematically investigated.

Experiments on rivalry have studied its rate (the number of changes per unit time) more often than predominance. Breese (1899 and 1909) found that if the illumination of both his red and green squares was increased, then the rivalry was faster, and Lawaetz (1939) found the same when the luminance of both his slits of light was increased. These results, together with those in the previous paragraph, are analogous to the effects of blurring the rivalling stimuli: Fry (1936) found that blurring one stimulus made it less predominant and Breese (1909) found that blurring both slowed the rivalry. I have confirmed both these results in an experiment not reported here. Thus the following generalization is plausible: Suppose that the rivalling stimuli can be measured along some dimension X. If increasing X for one stimulus makes that stimulus predominate, then increasing X for both, but keeping predominance unchanged, will make the rivalry faster. The qualification "keeping predominance unchanged" is necessary because rate is unlikely to be independent of predominance. For example, when predominance is complete the rate is zero. Evidence relevant to these claims will be presented. However, in the following, predominance has been studied more than rate, because the results are simpler to analyse when only one stimulus is varied at a time, and this affects predominance more than rate.

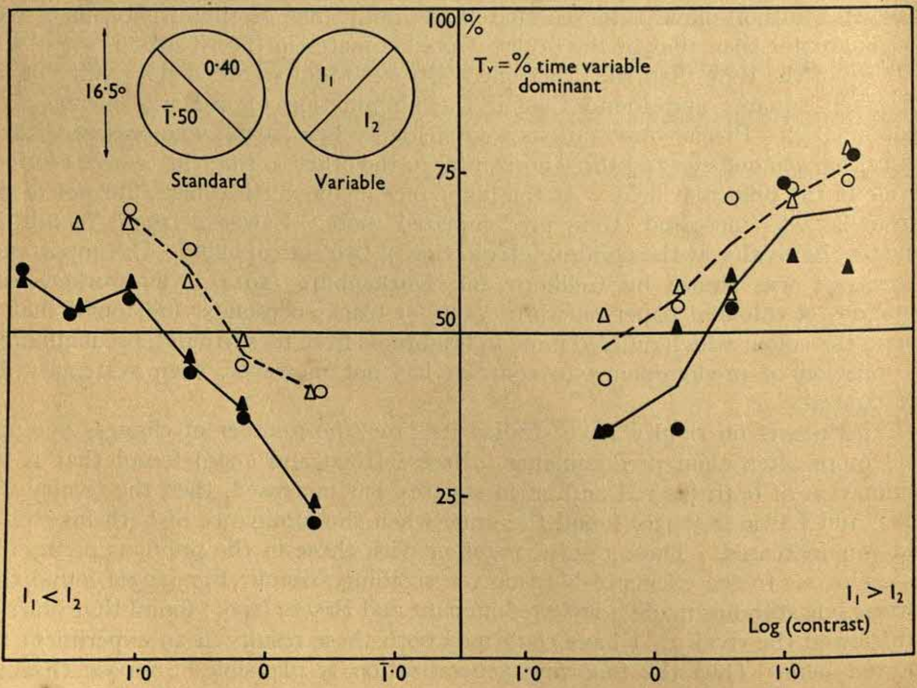
My first experiment used the rivalry of a single pair of intersecting contours, since this rivalry seemed the most likely to show a clear dependence on the contrasts at the two contours.

EXPERIMENT I. THE RIVALRY OF INTERSECTING CONTOURS

Displays and apparatus

The displays are shown in the inset to Figure 1. When these are superimposed in a stereoscope, the diagonal contours rival at their point of intersection. The "standard" display, with the luminances shown (Log ft.-L) was seen by one or the other of the subjects' eyes throughout. The luminances of the display seen by the other eye were varied: "I_2" was 0·50 or 1·00 Log ft.-L, while "I_1" had various values from $\bar{2}$·50 to +2·50 Log ft.-L. The diagonal contours were at 44 cm., and the boundaries of the fields at 14 cm., from the eyes. The two halves of each monocular display were vertical aluminium sheets coated in magnesium oxide, one (luminance I_1) behind the other (I_2). The upper edge of the one nearer the subject (I_2) was sharp, so that it could not be seen highlighted or shadowed. These two screens were seen through a circular hole in a sheet of black cardboard. They were illuminated by tungsten bulbs through apertures whose width could be altered to vary the illuminations. The luminances were always adjusted and

FIGURE 1



The display and results of Experiment I. Dashed lines and hollow symbols are results for I_2 = 1·00 Log ft.-L; full lines, filled symbols, for I_2 = 0·50. Triangles show results for subject PL, circles for DB. The lines go through the averages.

checked with the aid of an "S.E.I." photometer, because the apertures could not be reset accurately. The bulbs were run through a variable transformer which the experimenter adjusted to compensate for changes in mains voltage. The evenness of the illumination and the sharpness of the diagonal edge was shown by the fact that when the two screens were at the same luminance the edge could not be detected for the greater part of its length. The subject saw the displays reflected in vertical front-silvered mirrors close to his eyes, so arranged that the displays appeared superimposed in a frontal plane and in the centre of his field of view. He looked through artificial pupils 2 mm. in diameter. His head was fixed by a bite-block and a forehead rest.

Procedure

Subjects were asked to fixate the point of intersection of the diagonals, the displays being adjusted for each person so that they remained superimposed when he fixated. Two microswitches were comfortably placed under the subject's hand: one to be pressed when the diagonal from top-left to bottom-right appeared continuous across the point of intersection, and the other when the other diagonal did. On occasions when both appeared continuous, one always looked sharper and/or in front of the other, and the switch corresponding to that one was to be pressed. The microswitches were each connected to a solenoid-operated stop-clock which thus registered the total time for which the corresponding diagonal had dominated. Counters recorded the number of times the switches were pressed. In all the experiments described in this paper, subjects were asked not to try to control the rivalry, but to observe it as "passively" as possible.

Previous experiments had suggested that meaningful results could be obtained only by averaging several trials, so that more than one session was required; and also that session to session changes, possibly in eye-dominance, occurred. This experiment was therefore rather cumbersomely designed to evaluate and compensate for these changes, although good results were later obtained in single sessions, with experienced subjects. Only one or two values of the luminances of the variable display were used in each session. Rivalry was recorded for a block of four or five trials with the variable to the left eye, say, and the standard to the right, and then for the same number of trials with the displays changed round. This made it possible to average out the effects of eye-dominance, and also to see how large those effects were. The results will be expressed in terms of the average percentage of viewing time for which the variable stimulus was dominant ("T_V"). The data for the two blocks of trials were in fact averaged by taking the geometric mean of the ratios of the times for which the variable and the standard were dominant in each block. However, the scatter of results was such that the precise method of averaging was not important.

Except in some special trials (see below), the contours always ran upper-temporal to lower-nasal in each visual field.

Trials lasted about a minute, with pauses of a minute between the trials of one block. Before each block subjects adapted to the luminance level of the displays by looking at them for 3 min. while deliberately avoiding fixation. Two subjects were used, neither of whom knew the purpose of the experiment, nor had had previous experience in such experiments.

RESULTS

Predominance and contrast

In Figure 1 the percentage of time for which the variable was dominant is plotted against Log (contrast). "Contrast" = $(I_2 - I_1)/I_1$ when $I_2 > I_1$, or $(I_1 - I_2)/I_2$ when $I_2 < I_1$. The lines are the averaged results of two subjects, the points are results for each individual. When the same display was presented in more than one session to the same subject, to check on reliability and eye-dominance, the result given is the average.

It can be seen that T_V increases with contrast for constant I_2, and with I_2 (and hence with average luminance) for constant contrast. The latter was shown by the fact that the dashed curves, which are for the higher value of I_2, are above the solid ones, and also by the fact that the curves on the right are above the corresponding ones on the left.

The rate of rivalry was very variable and showed no clear dependence on the luminances.

Eye-dominance and intersessional changes

Although both subjects were fairly consistently right-eye dominant, the amount of eye-dominance varied considerably from display to display in the same session, and, more, from session to session with the same display.

Since the contours always ran from upper-temporal to lower nasal in the visual fields, it was possible that eye-dominance was confounded with what might better be called "direction-preference." To test this, special sessions were given in which each eye saw the contour at right angles to its usual direction in some trials and in the usual direction in others. It appeared that both subjects had both direction preference and eye-dominance. This was after about 20 sessions with the displays in their usual orientations.

Even when the effects of eye-dominance, etc., were averaged out, T_v varied significantly from session to session for the same display. The size of these variations were of the order of the irregularities that can be seen in the results in Figure 1.

The changes in T_v and in eye-dominance seemed both independent and random. That is, they were not related to each other, seemed independent of the particular displays seen, and showed no long-term trends. The rate of rivalry, on the other hand, did show long-term changes. For subject PL it started at about 60 changes/min. and decreased irregularly throughout the experiment to about 40, while for DB it started at 20 and increased to 40. Consistent changes did not occur within a session.

Experiment II. The Rivalry of Light and Dark Circles (1)

Since the rivalry in the previous experiment was so plainly between contours, it is hardly surprising that predominance depended on the contrasts at those contours. This would be a less obvious result for the rivalry of small congruent figures like the circles in the display of this experiment (Figure 2). However, in this case also the rivalry depends on the relative positions of the contours, e.g. if one circle is much larger than the other there is little rivalry. Further, it is already known that in one sense the rivalry between congruent uniform regions depends on the contrast at their contours. For, with this type of achromatic display rivalry occurs if one circle is darker than its surround and the other lighter, but not if both are darker or both lighter. This important but little known result has been noted by Fry and Bartley (1933) and Treisman (1962). Treisman's results and those of the following experiments are strong evidence in support of the first part of the above result. The second, that there is no rivalry when both figures are darker or both lighter than their surrounds, is shown for surrounds of equal luminance by, for example, Fry and Bartley's experiment, but more evidence is required for the case in which the luminance of the surrounds are different. Informal experiments of my own, with four subjects, confirmed it, although they did not investigate extremes of contrast, nor differences between the surrounds of more than 1·5 log units. But the result holds for all conditions so far investigated. In the following experiment contrast was varied over some of the range in which there is rivalry.

Displays and apparatus

The displays, shown in the inset to Figure 2, were presented in the stereoscope used in Experiment I. The surround was an aluminium screen coated in MgO, with a hole in it to form the small central circle, through which another coated screen could be seen. The small circles were at 44 cm., and the boundaries of the fields at 14 cm. from the eyes. The edges of the hole were bevelled to eliminate any shadowed outline, so that when the luminances of the surround and circle were the same the latter was invisible. Artificial pupils, bite-block and mains voltage adjustment were as in Experiment I.

Since circles both lighter or both darker than their surrounds do not rival, two "standard" displays were used, a dark one ($\bar{1}$·27 on 0·18 Log ft.-L surround) to rival with variable light circles and a light one (0·56 on 0·18) for variable dark circles. Some trials were also run using a standard dark circle of lower contrast (1·11 on 1·35) than the usual.

Procedure

No attempt was made to evaluate eye-dominance or long-term changes in time. The variable was always presented to the left eye and rivalry was recorded with all values of it in one session, in random order. In some sessions more than one trial was given with each display (not successively) to make some estimate of repeatability. Trials lasted a minute with rests of a minute in between.

For high contrast stimuli, varying the luminances by means of variable apertures, as in Experiment I, was adequate. For finer adjustments a variable transformer in series with the tungsten bulbs was used. By including a blue bulb among the tungsten ones, the colour changes produced by this method were made smaller than the apparent colour differences between the two eyes which most subjects experience. Accurate settings of much lower contrast than was used in Experiment I, were achieved by having the subject adjust the variable transformer until the circle disappeared, and then resetting it a certain number of volts above the disappearance value. The system was calibrated with an SEI photometer and checked several times during the experiments. The variations in the settings were small compared with those which would be required to account for the variations in the subjects' responses.

Subjects were asked to fixate the small dot to the right of the circles. They were again given two microswitches, and asked to press one when more of the light circle was visible than of the dark, and the other otherwise. The two circles were never equally visible except instantaneously or when they separated because of changes in the vergence of the eyes. In the latter case subjects were to cease responding until the circles were again superimposed.

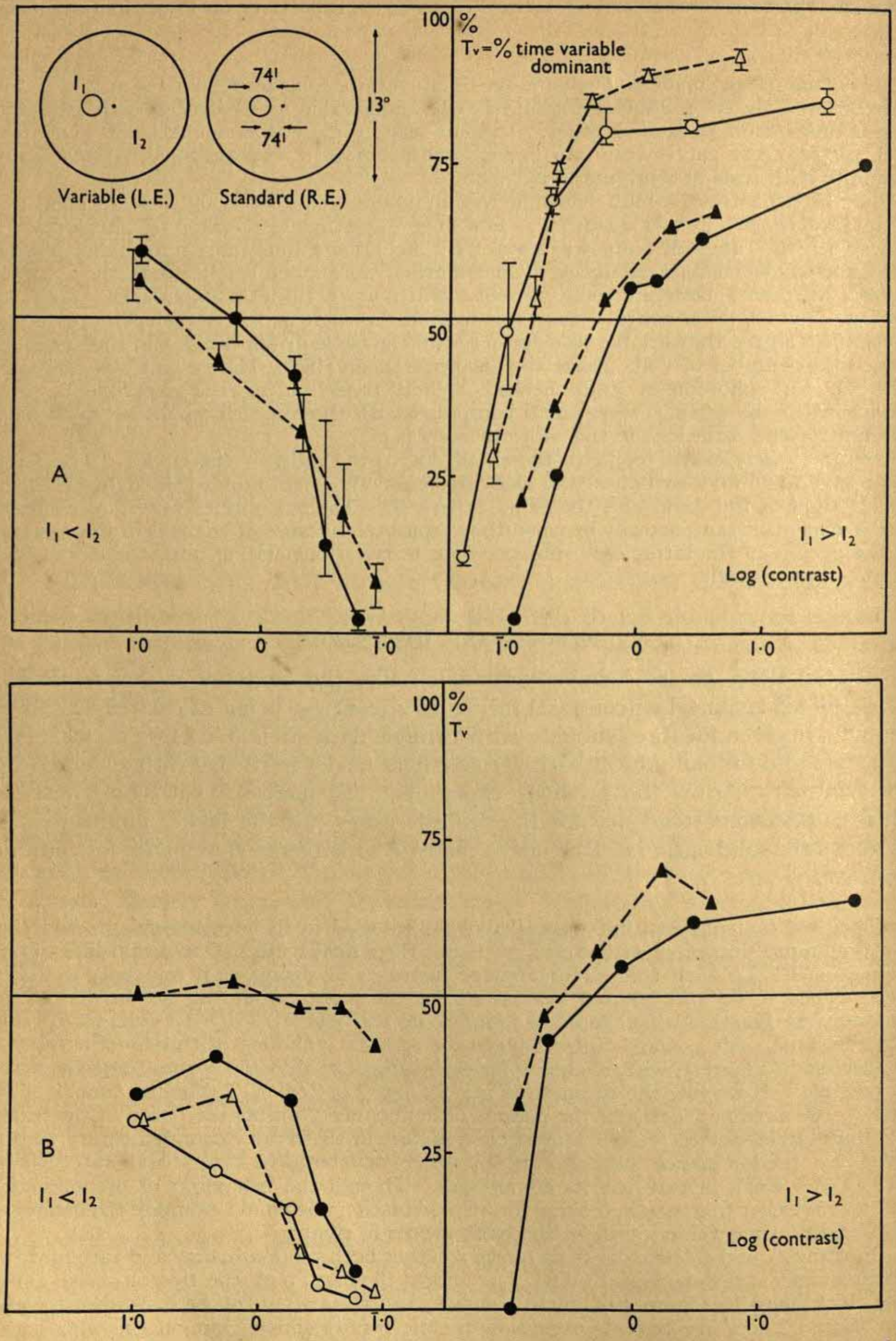
RESULTS AND DISCUSSIONS

The rivalling circles behave very much like the contours in Experiment I. T_V is plotted against Log(contrast) in Figure 2 (contrast being calculated as above). Results are given for three subjects with variable dark circles, and for two with light ones, and also for one subject with variable light circles in rivalry with a standard of low contrast. As in the previous experiment, T_V increases with Log(contrast). There is also some tendency for the dashed curves (for the higher luminances) to lie above the solid ones; i.e. T_V tends to increase with average luminance for constant contrast.

This last fact implies that when "contrast" is used in its precise sense, to say that "predominance increases with contrast" is not the whole truth. One would like to find a function $f(I_1,I_2)$ such that predominance increases or decreases if and only if $f(I_1,I_2)$ does. Then if T_V were plotted against $f(I_1,I_2)$ the points would all lie on one curve. The simplest function which could be found to do this was $(I_1 - I_2)/(I_2 + K)$ for $I_1 > I_2$, and the same with I_1 and I_2 interchanged for $I_1 < I_2$. The log of this function is used as abscissa in Figure 3, with K chosen for each subject so that the curves coincide as well as possible. However, the ordinate is not T_V but "t_b" or "t_d" ("bright time," "dark time"), the average lengths of the periods of dominance ("on-phases," say) of the bright and dark circles respectively. These give a picture of the rivalry complementary to that given by predominance ($T_V = 100t_b/(t_b + t_d)$, or $100t_d/(t_b + t_d)$) and rate ($= 60/(t_b + t_d)$). Each picture has its advantages. Though the behaviour of predominance and rate is easier to grasp, in looking for an explanation one would examine the behaviour of t_b and t_d, since these represent the basic events in rivalry.

Plotting t_b and t_d also allows us to see whether both predominance and rate, not just predominance, are determined by $f(I_1,I_2)$. For if they are, both the two on-phase curves (the thick ones) in Figure 3 ought to coincide, and the two off-phase ones also, for each condition. There are obvious exceptions in the results shown, but on the whole these are not greater than the departures from smoothness of the individual curves. If one assumes that these departures depend on some factors other than the luminances; then they are not important to a general picture of the influence of contrast and luminance on rivalry. However, more data are desirable.

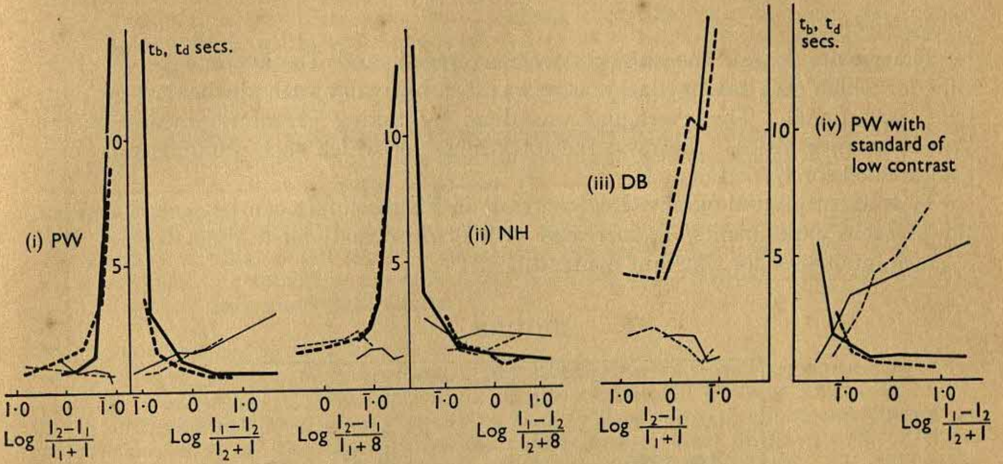
FIGURE 2



Displays and results of Experiment II. Dashed lines and triangles are results for $I_2 = 1{\cdot}66$ Log. ft.-L; full lines and circles for $I_2 = 0{\cdot}25$. (A) Subject PW. Hollow symbols are results with a standard of low contrast. (B) Subjects NH (filled symbols) and DB (hollow symbols).

It is striking that altering the contrast of the variable has much more effect on its off-phase than on its on-phase, except when the standard is of low contrast. The lengthening of the off-phase when the contrast is low represents, of course, the transition towards complete predominance of the standard over the uniform field left when the variable is of zero contrast.

FIGURE 3



The results in Figure 2 plotted in a different way (see text). Dashed lines are results for $I_2 = 1{\cdot}66$ Log ft.-L, full ones for $I_2 = 0{\cdot}25$. Thick lines show the length of the on-phase of the variable, thin lines the off-phase.

EXPERIMENT III. THE RIVALRY OF BRIGHT AND DARK CIRCLES (2)

This experiment was done before the previous two, before I had realized the importance of using very low values of contrast to produce large changes in predominance. It used 20 subjects, to cope with the large inter- and intra-personal variations which had occurred in preliminary work. It remains of value in showing that results like those of Experiment II can be obtained by averaging the responses of many naïve subjects, in showing the range of results to be expected, and in making possible some generalizations about the rate and the subjective appearance of rivalry.

Displays and apparatus

The display was that of Experiment II, presented in the same stereoscope, but without artificial pupils or bite-block. There were three surround luminances, $\bar{1}$·0, 0·5 and 2·0 Log ft.-L, with the small rivalling circles either slightly (0·1) or much (about 1·0 Log ft.-L) brighter or dimmer than the surround, except that the apparatus did not allow one much brighter than the brightest surround. Each of the 30 possible pairs of displays were presented to each subject. A "Gregory-Russell" recorder registered the time and type of each button-press, so that the records contained a great deal of information, which could be analysed in many ways.

Procedure

The subjects' instructions differed from those in Experiment II in some respects: they were given three buttons to record the rivalry, one to be pressed when a whole bright circle was visible, one for a whole dark one, and one if a combination of the two was seen; they were asked to fixate the centre of the circle, not the small dot; and after each trial they were asked some questions about the rivalry, which will be referred to below. The displays were presented in a random order to each subject. There was one 80-sec. trial per display, from the record of which a 60-sec. portion, not including the first few button presses, was selected for analysis. Inter-trial intervals consisted of 90 sec. rest in the dark and then 30 sec. adaptation to the next display luminance, looking into the stereoscope and deliberately avoiding fixation.

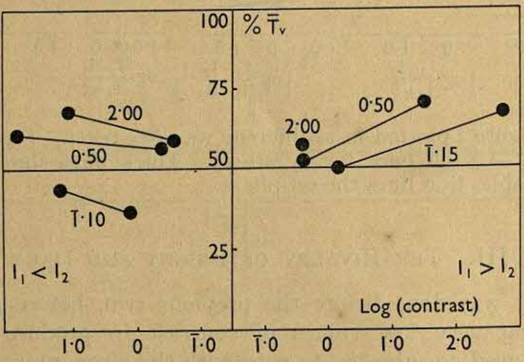
RESULTS AND DISCUSSION

Predominance

The results for all the subjects were averaged, and $\bar{T}_V$ (average per cent. of time for which each bright (dark) circle was seen in rivalry with all the dark (bright) ones) was found. The averaging was done by taking geometric means of the "predominance ratios," the ratio the total times for which each stimulus was seen, as described for Experiment I.

$\bar{T}_V$ is again plotted against Log(contrast) in Figure 4. It can be seen that as in the previous experiments, $\bar{T}_V$ increases with Log(contrast) for a given I_2, and with average luminance for constant contrast.

FIGURE 4



The results of Experiment III. Averages for 20 subjects. The lines join points representing displays with the same value of I_2, the value being indicated in Log ft.-L.

Rate of rivalry

The rate of rivalry varied more between subjects than between displays, in that the inter-subject range of averages-over-displays was from 12 to 42 changes/min. while the inter-display range of averages-over-subjects was from 12 to 34. However, the average rate for a display-pair was greater, the more evenly matched the rivalling stimuli. That is, rate correlated negatively over the 30 display-pairs with the predominance ratio (taking whichever value of this ratio is > 1). Spearman's Rank Correlation Coefficient is $r_S = -0{\cdot}52$, for which a one-tailed p is $< 0{\cdot}005$. Further, the rivalry was faster, the stronger were the competing stimuli, if we take $\bar{T}_V$ as a measure of the strength of each stimulus. That is, rate correlates positively with $(\bar{T}_{V1} + \bar{T}_{V2})$, where $\bar{T}_{V1}$ and $\bar{T}_{V2}$ are the values of $\bar{T}_V$ for each monocular display. $r_S = +0{\cdot}56$, $p < 0{\cdot}005$. These two correlations were independent since $(\bar{T}_{V1} + \bar{T}_{V2})$ and the predominance ratio are not correlated, positively or negatively.

These results support the generalizations made about rate in the introduction. The variability of the results was such that it is not worthwhile to discuss the pros and cons of $(\bar{T}_{V1} + \bar{T}_{V2})$ as a measure of total stimulus strength. Other measures which were tried, correlated with rate to about the same extent.

"*Mixture*"

As with rate, the average amount of mixture seen varied more between subjects (0·3 to 51·2 per cent. of the time, averaged over all displays) than between displays (1·1 to 29·5 per cent. averaged over subjects). Subjects were asked after each trial to describe the appearances of the mixture which had occurred. The commonest reports were that it looked like a patchy mixture of the two circles (31 per cent. of all reports), or that both circles were visible in the same place (37 per cent.). This latter report was more frequent when there was an apparent size-difference between the circles ("irradiation"). Sometimes the circles moved so as to be no longer exactly concentric (15 per cent.), or fused to a homogeneous mixture (10 per cent.). Lustre was occasionally reported (2 per cent.), and the remaining 5 per cent. contained such reports as complete disappearance of both circles.

It was possible to analyse the frequencies with which mixture occurred in various sequences in the rivalry. E.g. as a transitional phase between dominance of the dark and bright circles or as a partial appearance of one circle which would then disappear without having become completely dominant. The results of this analysis were in accord with common sense, and although they are not relevant here, that they could be obtained at all shows in what detail rivalry experiments can give meaningful results, even with naïve subjects. It should be mentioned that all these mixture results were symmetrical with respect to dark and bright circles.

Binocular lustre

That lustre was so rarely reported might surprise anyone who has noticed that the displays used satisfy the descriptions of figures which will produce binocular lustre, given by Helmholtz (1856, **3**, p. 512) and by Bartley (1941, p. 51). Lustre was, however, perceived almost all the time (in informal experiments) with displays such as those illustrated by Helmholtz: *outline* drawings in black on white to one eye and vice versa to the other, or alternatively with a "filled-in" figure, such as a uniform bright or dark circle, to one eye and a congruent outline, black or white, to the other.

This fact may be seen as an interesting extension of the result stated in the introduction to Experiment II, that congruent figures fuse binocularly if their contours are of the same sign (i.e. if the step in luminance across the contours is an increment from left to right, say, in both eyes, or a decrement in both), but rival if the contours are of opposite sign. For, in the situations which give lustre there are contours in corresponding positions to the two eyes which are of the same sign, but if these were fused there would then be an additional contour, seen by one eye only, on one or both sides of the fused contour. Lustre is correspondingly perceived on one or both sides of the group of contours. Lustre thus occurs when stimuli to both rivalry (contours of opposite sign) and fusion (contours of the same sign) are presented.

DISCUSSION

The similarity of the relations of predominance to contrast for intersecting contours (Experiment I) and for uniform regions (Experiments II and III), and the dependence of rivalry in the latter case on the relative positions of the contours, suggests that in this case the rivalry is between neural events in the parts of the visual system stimulated by the contours. Other lines of evidence also suggest that the perception of a uniform region, at any rate under steady fixation, depends on activity initiated by its contours. For example, the fact that a "stabilized retinal image" disappears more than an object which is merely fixated as steadily as possible, and, in neurophysiology, the evidence that cells in the visual cortex (of the cat) are activated almost only if a contour falls in their receptive field (e.g. Hubel and Wiesel (1962)). Do other aspects of perception depend on contrast in the same way as does predominance?

Brightness is suitable for comparison with predominance, because it can be tested under the same conditions of steady fixation etc. as rivalry, and also because we can already see that under some conditions it does depend on contrast in approximately the same way as predominance does:—

Compare the results in this paper with those of Heinemann (1955) on simultaneous contrast. Heinemann presented a circular patch in an annular surround to each eye, so that the two displays were seen side by side in the binocular field. For a given pair of patch and surround luminances in one eye, he asked his subjects to adjust the luminances of the patch (I_1) in the other eye, for various surround luminances (I_2), to make the patches look equally bright. He found that patches with contours of opposite sign (see above under "Binocular lustre") were almost never matched, and the results for bright circles ($I_1 > I_2$) are approximately (although there are some consistent discrepancies) a mirror-image of those for dark ones ($I_1 < I_2$). That is, brightness shows a division and a symmetry between the results for dark and bright circles, as does predominance. Further, Heinemann's results show that for constant contrast both brightness ($I_1 > I_2$) and darkness ($I_1 < I_2$) increase with average luminance (although darkness does not at high luminances) as predominance does, and that for constant ($I_1 - I_2$) the opposite is true, as it is for predominance. The latter can be shown for the results of the experiments in this paper by replotting them with $\text{Log}(I_1 - I_2)$ as abscissa, which makes the curves for the higher average luminance lie below the others, with no exceptions. Thus the relations of both brightness and predominance to I_1 and I_2 are closely similar. It remains to enquire whether, for the same subject at a given time with a given display, two patches which are equally predominant, over some third stimulus seen by the other eye, are also equally bright. If this were so, it would suggest that the value of some single function of the two luminances on either side of a contour was the only information about luminance, as distinct from form etc., used by the visual system, and would lead to attractively simple models of this aspect of perception.

In a preliminary test of this notion, with only the author as subject, equally bright patches were not equally predominant. Of two equally bright circles, the one with the greater brightness difference between itself and its surround was always the more predominant in rivalry. More work is being done on this question.

This work was done while I held a Scholarship from the Medical Research Council. I am grateful to Mr. G. C. Grindley for assistance, advice and patience, and to Professor O. L. Zangwill for facilities and encouragement.

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THE NATURE OF THE AGE DECREMENT IN PERFORMANCE ON DICHOTIC LISTENING TASKS

BY

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Previous experimental work has shown that when subjects of different ages are presented with a series of dichotic digits (as two simultaneous half-sets, one to each ear) there is an age decrement in the reproduction of the second half-set. It has been suggested that this result is due to the second half-set being involved in a short-term storage process and that this process declines in efficiency with age.

It seemed equally possible however that the result was due to perceptual or attentional factors rather than memory factors. Two experiments were thus carried out in an attempt to specify the nature of the decrement. In the first, instructions as to which half-set to reproduce first were given before presentation of the digits. In the second experiment attention was equally divided between the ears by giving these instructions after presentation. An analysis of types of error in the first experiment suggested that attentional factors were more important but from a similar analysis in the second experiment it was concluded that memory factors were largely responsible for the age decrement.

INTRODUCTION

It has been suggested (Welford, 1956) that a decline in the efficiency of short-term memory with age may account for the poorer performance of old subjects on learning and problem-solving tasks. However, Gilbert (1941) and Bromley (1958) have shown that there is very little decline with age in an orthodox "digit-span forward" test. This test might be regarded as a fairly pure measure of short-term memory so that these results seem at first sight to contradict Welford's point of view. On the basis of further experimental evidence, however, Welford (1958) concluded that simultaneous activity such as rehearsal or recall of some of the memorized material interferes with the retention of the rest and that this interference effect may become progressively greater with age. Thus in situations where the memorized material is within the short-term span no age decrement in short-term memory should appear. On the other hand an age decrement would be expected in tasks which demand the simultaneous occurrence of storage and other activities.

Recently Inglis and Caird (1963) have presented subjects of different ages with such a task. Digits were recorded on tape and then delivered dichotically so that the subjects (who were wearing earphones) heard two simultaneous half-sets, one half-set in each ear. Messages varied in length from one to six digits per half-set and the rate of presentation was 90 pairs of digits per min. Subjects were instructed: "Tell me what numbers you hear" and so attempted to reproduce the message after presentation. At this speed of presentation subjects almost invariably recall one complete half-set before recalling the other; the score obtained was the average number of digits correct (taking position into account) in each half-set. No age decrement was found in the half-set recalled first but there was a marked age decrement in the reproduction of the second half-set. This result has since been confirmed by Mackay and Inglis (1963).

Rather than invoke increased interference as the cause of the observed decrement Inglis and his associates prefer to explain their result in terms of Broadbent's (1958) information-flow model. They suggest that the half-set of digits recalled first passes directly into the "p-system" of Broadbent's model while the half-set recalled second spends some time in a short-term store (the "s-system") before passing to the p-system. It is suggested further that the efficiency of the short-term store declines with age. Thus, as the half-set recalled first does not involve this process it shows no decline with age; the half-set recalled second, however, shows a marked decline.

It was felt that the free recall instructions used in these experiments would probably give a bias to the half-set recalled first as the subject would recall that half-set which he remembered better. If the subject was instructed beforehand which half-set to reproduce first it would make the task more specific and might reveal more about the processes involved. Thus Inglis and Caird's experiment was repeated with that modification and also two rates of presentation were used: 30 pairs of digits per min. and 90 pairs per min. The slower rate should exaggerate any errors due to decay of the memory trace and, in addition, while it seemed just possible that at 90 per min. the second half-set of digits remains in a pre-perceptual store until the first half-set is reproduced (as Inglis and Caird suggest) this seemed impossible for pairs of digits arriving at 2 sec. intervals. In terms of Broadbent's model each pair of digits must be perceived and then passed to the storage system while the p-system deals with the next pair. This process might give rise to different results from those observed by Inglis and his associates.

EXPERIMENT I

Procedure

Two lists of six-digit messages were pre-recorded on a two-channel tape recorder, one list at 30 pairs of digits per min. and the other at 90 pairs per min. Each message was recorded with three digits on one channel and three on the other so that when played back through earphones three pairs of digits were heard, the members of each pair arriving simultaneously in the right and left ears. At each speed there were 10 scored messages preceded by three messages for practice.

The set-up was explained to the subject and he was told that after hearing the message he must attempt to recall all six digits, all three from one ear in their correct order and then all three from the other ear. He was instructed beforehand which half-set (left ear or right ear) to reproduce first and was encouraged to give six digits in all cases and to guess if he was not certain. In each age group subjects were balanced for which speed condition they did first and also for which half-set they were requested to reproduce first; (half the subjects were asked to give "right ear first" throughout, the other half "left ear first"). The score obtained was the average number of digits correct per half-set taking position into account.

Subjects

The subjects used were 48 persons drawn from a volunteer panel. Four men and four women represented each decade age group from the 20's to 70's and each group of four consisted of two "average" subjects scoring 25–28 in the Mill Hill Vocabulary test and two "high" subjects scoring 37–40. Thus the age groups were matched on verbal intelligence—a measure which is known to decline little with age.

RESULTS AND DISCUSSION

The results are shown in Figure 1 and the slopes were examined for differences from zero slope by trend analysis of variance (Table I*a*). This table shows that at both speeds performance on both Channel 1 (the half-set recalled first) and Channel 2 (the half-set recalled second) declines significantly. Further that the difference between the slopes is significant at 30 per min. but not at 90 per min.

TABLE I

TREND ANALYSES OF VARIANCE ON DATA FROM EXPERIMENT I

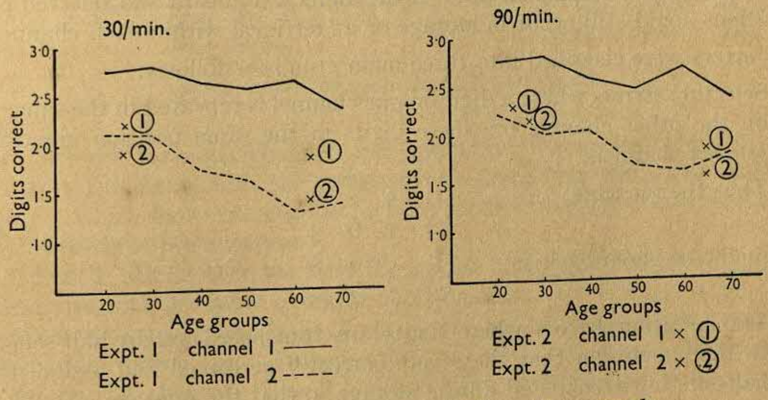
(a) Overall results		*F*	*p*
30 per min. . .	Slope (Channel 1)	11·30	<0·01
	Slope (Channel 2)	15·87	<0·001
	Difference between slopes	4·66	<0·05
	Difference between intercepts	117·90	<0·001
90 per min. . .	Slope (Channel 1)	8·18	<0·01
	Slope (Channel 2)	5·78	<0·05
	Difference between slopes	0·81	n.s.
	Difference between intercepts	53·50	<0·001

(b) Errors		*Channel 1*		*Channel 2*	
		F	*p*	*F*	*p*
30 per min.	Selection . .	2·36	n.s.	2·66	n.s.
	Transposition	1·50	n.s.	0·55	n.s.
	Random . .	9·56	<0·01	16·26	<0·001
90 per min.	Selection . .	4·78	<0·05	2·59	n.s.
	Transposition	2·17	n.s.	0·41	n.s.
	Random . .	5·23	<0·05	4·74	<0·05

Probabilities greater than 0·05 are indicated as non-significant (n.s.).

The finding that performance on Channel 2 declines with age confirms the result of Inglis and Caird but the finding that performance on Channel 1 also declines with age is contrary to their results. This discrepancy between the experiments seems most probably due to the different recall instructions given to subjects.

FIGURE 1



Recall of digits presented dichotically at two speeds.

The age decrement on Channel 1 observed here could be due to one or more of several factors. As mentioned above it seems highly unlikely that Channel 2 digits are held in a pre-perceptual store until Channel 1 has been reproduced. If digits from *both* channels are passed after perception to a short-term store then one would expect decay in both channels but more in Channel 2 digits as they are held longer. If rate of decay increases with age this would give rise to the observed result. Alternatively the result could be due to the increased effects of interference with age as Welford suggests. Again it is possible that the ability to correctly select which digits are coming into each ear decreases with age. It is held that any of these possible effects would affect both channels.

One of the effects of instructing a subject beforehand which ear to reproduce first appeared to be to largely concentrate his attention on that ear. That is, for all subjects attention was fixed on Channel 1 throughout the experiment. If this was true it is difficult to say whether the lower performance on Channel 2 is due to this factor or due to a memory decrement as Inglis suggests. If subjects are attending to one ear they may simply not hear so well with the other. This explanation is given added weight by the results of Broadbent and Gregory (1963) who concluded from a study using young subjects that diversion of attention away from a stimulus produces an effect resembling a reduction in the intensity of the stimulus. This division of attention factor might become more pronounced with age so that the decrement on Channel 2 could be explained as being due to lowered effective signal strength. That is, the decrement might be perceptual rather than due to memory.

At this stage the process was envisaged as follows:

(*a*) provided the effective signal strength was great enough for the digit to be correctly perceived then
(*b*) it would be selected as being appropriate to Channel 1 or 2 and
(*c*) stored until
(*d*) each channel was "retrieved" from store and reproduced.

In order to specify at which stage of the process errors were occurring it was considered likely that difficulties at each stage would give rise to different *types* of error. For example, if the subject did not hear the digit properly he would be obliged to either give a digit at random, omit that digit or possibly make an "acoustic confusion" error (e.g. 9 for 5). If there was difficulty in the selection process he would allocate the digit to the wrong channel and possibly cross it over with the digit from the opposite ear. If the digits were heard and selected properly, transpositions could still occur in storage or at retrieval within each channel group.

Thus errors were classified into three main groups as follows:

(*a*) Selection Errors where a digit on one channel is repeated in the same position on the other channel or where digits in the same position are transposed *between* channels.

Thus the message: (1) 5 2 8
(2) 1 6 3
might be given back as: (1) 5 2 8 or (1) 5 6 8
(2) 1 2 3 (2) 1 2 3

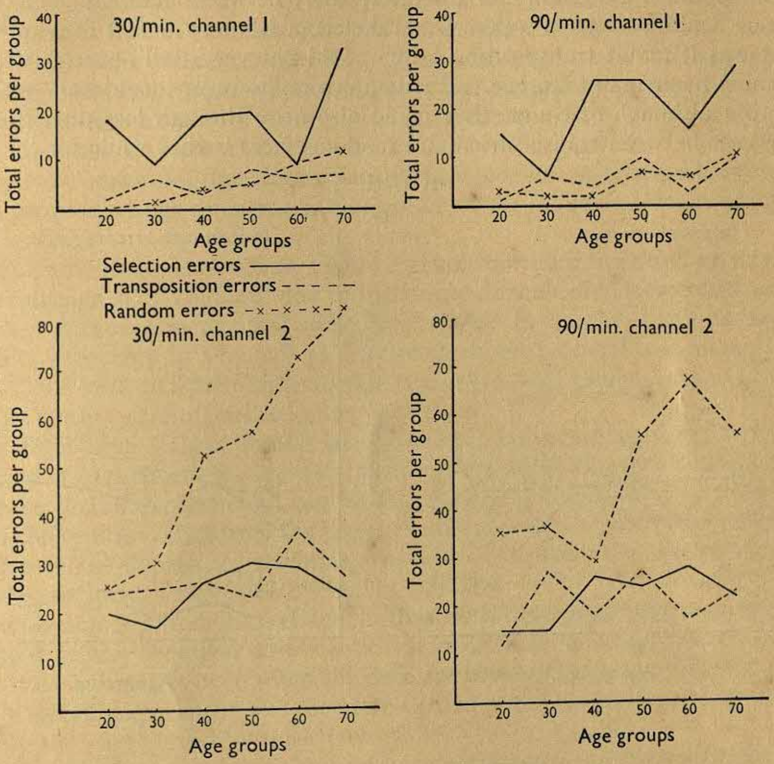
(*b*) Transposition Errors where digits are transposed *within* half-sets, leading to the conclusion that they were correctly perceived and assigned to their half-sets but transposed during storage so that the message:

(1) 5 2 8 might be given back as (1) 5 2 8
(2) 1 6 3 (2) 6 1 3

(*c*) Random errors—these are really "the rest" and are made up of omissions, acoustic confusions, mixtures of selection and transposition errors as well as apparently random intrusions.

By defining "Selection" and "Transposition" errors fairly strictly in this way it seemed possible to get quite pure measures of difficulty due to selection and short-term memory. Random errors are much less strict and would probably include selection and memory components but it seemed that this was the kind of error which would occur if effective signal strength was reduced.

FIGURE 2



Error scores for each channel and speed.

Error patterns for each channel and speed are shown in Figure 2. It was observed from this figure that the error patterns were very similar for both speeds so in Figure 3, which shows error scores for each type of error, the scores were averaged for the two speeds of presentation.

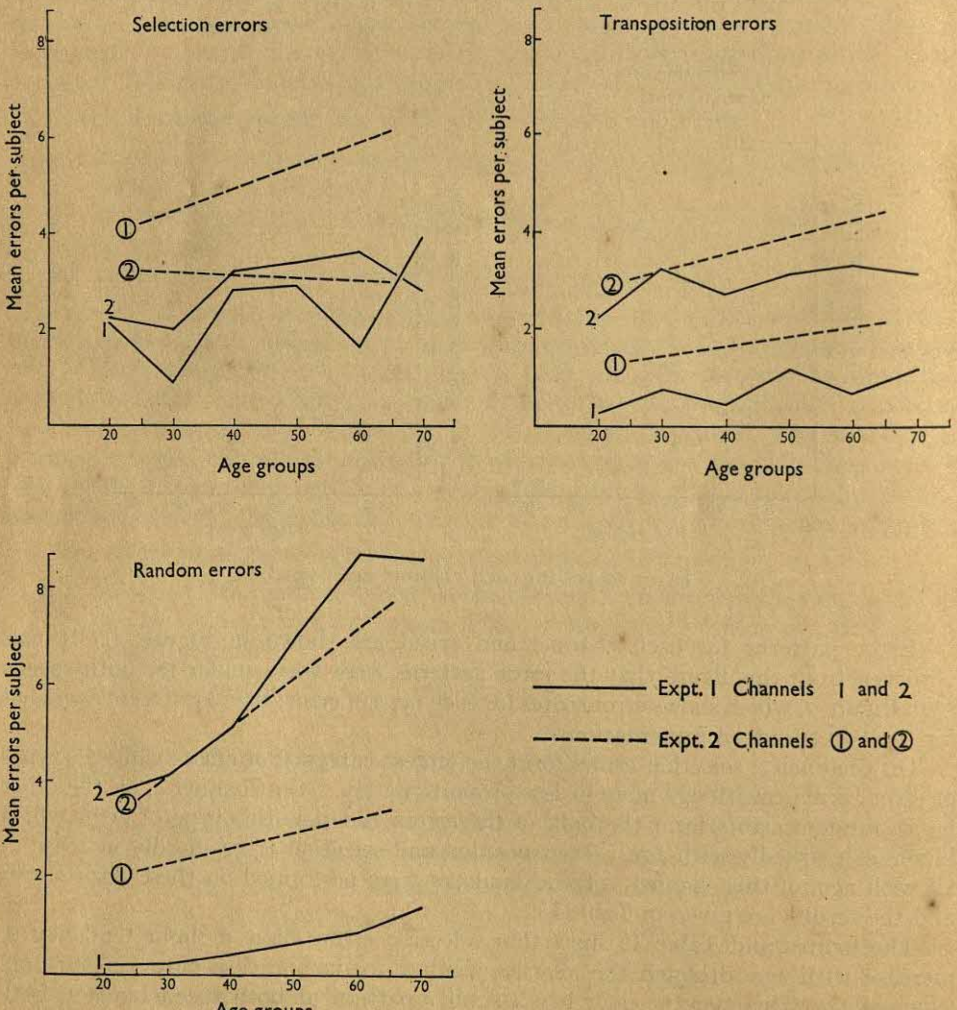
On Channel 1, selection errors form the largest category of errors while all types of error increase with age more or less proportionately. On Channel 2, on the other hand, random errors form the bulk of the errors and it is this type of error which increases markedly with age. Transposition and selection errors hardly increase at all with age on this channel. Trend analyses were performed on these error slopes and the results are given in Table I*b*.

The figures and Table I*b* show that selection errors have a slight tendency to increase with age although this reaches significance in one case only (90 per min. Channel 1). This type of error has a similar pattern at both speeds and on both channels suggesting that the locus of the errors is during input—presumably at stage (*b*) as outlined above. That is, the extra time the digits spend in memory in

Channel 2 or at the slower speed does not appear to have a very marked effect on the incidence of this type of error, therefore it seems most likely to occur during input.

For the transposition errors and the random errors, Channel 2 is worse than Channel 1 at both speeds. This effect is presumably due either to the fact that as attention was fixed on Channel 1, digits on Channel 2 were heard less well, thus less firmly registered and so more prone to changes during storage or that the extra time in store for Channel 2 accounts for the discrepancy between channels. That is, the discrepancy could arise from a perceptual deficit, a memory deficit or a mixture of both. It was felt that transposition errors could not very well occur during input so they must be errors of storage. Random errors, however, could very well occur during input although of course they could also arise through loss during storage. The difference between transposition and random errors is that while for the former

FIGURE 3



Error scores for each type of error in Experiments I and II (scores averaged for two speeds of presentation.)

class of errors there is no increase with age in the channel discrepancy there *is* such an increase with age for random errors. Therefore as transposition errors were judged to be a function of storage and show no increase with age in the difference between Channels 1 and 2 it was felt that random errors must be due to some different part of the process where such an age difference is operating—that is, that random errors are largely a function of decreased effective signal strength at input due presumably to the division of attention having a progressively greater adverse effect with age in the rejected ear.

On Channel 2 for both transposition and random errors, performance is somewhat worse at 30 per min. than at 90 per min. This adds support to the suggestion that transposition errors are errors due to storage but suggests at the same time that random errors may also be produced by storage faults. However, as transposition errors do not show a relative increase on Channel 2 with age while random errors do, it is tentatively concluded that the increase of random errors on Channel 2 is principally due to a progressive age difference in perception of the digits on the ear which is not being attended to.

To return to the overall effect, Figure 1 could now be interpreted as showing an age decrement in both channels due to increasing difficulties of perception, selection and storage although perceptual difficulties appear to be the major cause of the decrements especially in Channel 2. Performance on Channel 2 is worse than on Channel 1 due both to attention being diverted away from Channel 2 and to the fact that digits in that channel are held longer in store.

Thus Inglis and Caird's results have been partially confirmed. The fact that in the present experiment an age decrement was found on Channel 1 may be due to a difference in the experimental conditions. The major difference in the interpretation of the results is that from the present study it was concluded that a perceptual deficit due to the division of attention was a more important source of errors than immediate memory as suggested by Inglis and Caird.

It was concluded from Experiment 1 that both perceptual and memory factors contributed to the observed decrements. It is difficult to say, however, just how important each factor is even when the data are broken down into different types of error. A second experiment was therefore carried out in an attempt to specify more accurately the part played by these factors.

If the instructions as to which half-set to reproduce first are given *after* the subject has heard a dichotically presented message then there is no opportunity for him to consistently fix his attention on the half-set requested first. In this case he must divide his attention equally between the ears or arbitrarily fix it on one ear with the likelihood that the other half-set will be requested first on half the occasions. In this way any advantage due to attention which Channel 1 possessed in the first experiment would be cancelled out. If, on the other hand, the difference between the channels was due to the lapse of time between their reproductions, the discrepancy should still appear.

Thus repeating Experiment 1 with the difference that subjects would be instructed which half-set to give back first after presentation seemed to provide a way of differentiating between perceptual and memory factors. Further, the locus of the various types of error could be more firmly established using this procedure. If selection errors and random errors are due to switches in attention or loss of effective signal strength during input then under the "instructions after" condition no difference between Channels 1 and 2 should be found. Similarly if transposition errors are faults of storage then a channel discrepancy should still be observed.

A second experiment was thus undertaken to examine these hypotheses.

EXPERIMENT II

Procedure

The material used was the same as in Experiment I: messages were of six digits presented as simultaneous half-sets to each ear. The material was tape-recorded in two lists, one at 90 pairs of digits per min., the other at 30 pairs per min. and was played back to the subject through earphones. Each list consisted of 10 scored messages preceded by three messages for practice. Groups were again balanced for which speed they did first. Subjects were told immediately *after* presentation which half-set to reproduce first. Right and left ears were asked first five times each in random order for each list. The score obtained was again the average number of digits correct per half-set taking position into account.

Subjects

Two groups of subjects matched on verbal intelligence were used. All subjects were male, 20 comprised a Young group (age range 20–35; mean age = 28·2) and 20 an Old group (range 60–69; mean = 64·5). Mean Mill Hill Vocabulary scores were 35·3 and 36·1 for the Young and Old groups respectively. None of the subjects had participated in Experiment I and all were naïve in experiments on dichotic listening.

RESULTS AND DISCUSSION

Mean scores for each group are given in Table II and are also superimposed on Figure 1 for comparison with the first experiment. Table II also shows the results of t tests carried out between young and old groups for each channel and speed and also between Channels 1 and 2 for each age group and speed. These tests show that performance drops significantly with age on both Channel 1 and 2, thus confirming the finding in Experiment I, and also that performance on Channel 2 is significantly worse than on Channel 1 in three cases out of four. Figure 1 shows that the discrepancies between Channels 1 and 2 are greater for the old group at both speeds of presentation and also that the discrepancies for each age group are greater at 30 per min. than 90 per min. The significance of these differences between discrepancies was examined by taking each subject's channel discrepancy as a score and carrying out two sets of t tests. It was found that the difference did not reach significance in either case.

TABLE II

MEANS AND STANDARD DEVIATIONS OF OVERALL DATA FROM EXPERIMENT II. ALSO RESULTS OF t TESTS BETWEEN YOUNG AND OLD GROUPS AND CHANNELS 1 AND 2

	Young		*Old*		
	M	*SD*	*M*	*SD*	*t*
30—(1)	21·40	4·74	18·30	3·99	2·18*
30—(2)	18·80	4·84	14·05	5·98	2·69*
	t = 2·94**		t = 2·80*		
90—(1)	22·85	3·81	18·30	3·07	4·06**
90—(2)	21·65	4·79	15·35	5·34	3·91**
	t = 2·03		t = 2·52*		

* indicates $p < 0·05$
** indicates $p < 0·01$

A formula for correlated means was used in t tests between Channels 1 and 2.

Thus it has again been found that performance declines with age on both the first and second half-sets to be recalled. Figure 1 shows that in Experiment II the discrepancies between Channels 1 and 2 are very much less than in the first experiment. This suggests that the greater discrepancies obtained in Experiment I were caused by attention being on Channel 1 thus giving a lower effective signal strength on Channel 2. In the present experiment there is still some discrepancy between the channels, however, and these discrepancies are significant in three cases. This suggests that when the perceptual advantage to Channel 1 is removed, the discrepancy between Channels is reduced but that a memory factor still gives rise to poorer performance on Channel 2. The additional observations, that the discrepancies are greater in the old group and at the slower speed, while not significant still point to the memory decrement increasing with age and with a greater time lapse before recall.

Errors were classified as in Experiment I and the mean numbers of errors for each condition are shown in Table III. These data were analysed by t tests in the same manner as the overall scores for the experiment and a summary of this analysis is given in Table IV. This Table gives:

(*a*) The significance of differences between the young and old groups for both channels and both speeds of presentation, thus it can be seen which types of error increase significantly with age in each channel.

(*b*) The significance of differences between Channels 1 and 2 for both age groups and both speeds of presentation is given in Table IV*b*. According to the argument above the conditions of this experiment gave equal attention to each channel so significant differences between the channels are taken to indicate memory decrements.

(*c*) By taking each subject's channel discrepancy as a score it is possible to examine whether the differences between Channels 1 and 2 for each type of error are significantly greater in the old group. The results of this analysis are given in Table IV*c*.

(*d*) Similarly it is possible to examine, for each age group, whether the channel discrepancies for each type of error are significantly greater at 30 per min. than at 90 per min. This analysis is summarized in Table IV*d*.

TABLE III

MEAN NUMBERS OF ERRORS PER SUBJECT FOR EACH CONDITION IN EXPERIMENT II

		Selection		*Transposition*		*Random*	
		1	2	1	2	1	2
30 per min.	Young	4·3	3·0	1·6	3·4	2·5	4·8
	Old	6·0	2·6	1·9	4·8	3·7	8·4
90 per min.	Young	4·2	3·4	1·1	2·5	1·8	2·4
	Old	6·2	3·4	2·3	4·0	3·1	7·1

From Table III it is seen that in practically all instances the old group make more errors than the young group but this increase in errors with age is significant in only half the instances (Table IV*a*). The differences between Channels 1 and 2, on the other hand, are significant in all but one case (Table IV*b*). It was hypothesized

TABLE IV

SUMMARY OF RESULTS OF t TESTS ON DATA IN TABLE III

		Selection	*Transposition*	*Random*
(*a*) Age differences				
30 per min.	1	<0·05	n.s.	n.s.
	2	n.s.	n.s.	<0·01
90 per min.	1	<0·05	<0·05	<0·05
	2	n.s.	n.s.	<0·01
(*b*) Channel differences				
30 per min.	Young	<0·01	<0·05	<0·01
	Old	<0·01	<0·01	<0·01
90 per min.	Young	<0·05	<0·01	n.s.
	Old	<0·01	<0·01	<0·01
(*c*) Age differences in discrepancies				
30 per min.		<0·01	n.s.	<0·05
90 per min.		<0·01	n.s.	<0·01
(*d*) Speed differences in discrepancies				
	Young	n.s.	n.s.	<0·05
	Old	n.s.	n.s.	n.s.

Probabilities greater than 0·05 are indicated as non-significant (n.s.).
See text for explanation of this Table.

from Experiment I that transposition errors were a function of storage and would thus still show a channel discrepancy in the second experiment. It is seen from Table IV*b* that this hypothesis is confirmed. It was also hypothesized from the first experiment that selection and random errors were due principally to perceptual and attentional factors and would show no channel discrepancy when these differences between the channels were apparently removed. It is seen from Table III that this hypothesis is not upheld. Random errors are greater on Channel 2 for all conditions suggesting that these are storage errors also. Selection errors are in all cases significantly greater on *Channel 1* which seems at first a curious result bearing in mind that attention was equally divided between the channels. One possible explanation of this result is that selection errors are errors of retrieval; e.g. the subject may search for the three appropriate digits one of which may have decayed too much for retrieval, he thus selects the opposite channel digit from the same position as being the most likely and having used it is forced to substitute a random digit when giving Channel 2. This would explain the greater number of selection errors on Channel 1 and also explain to some extent the greater number of random errors on Channel 2.

When the age differences in channel discrepancies are considered it can be seen from Table III that the greater number of errors for the old group is always in the same channel as for the young group (i.e. greater in Channel 1 for selection errors and Channel 2 for transposition and random errors) and that the channel discrepancies are greater for the old group in all cases. Thus age appears to exaggerate the same types of fault found in the young group. From Table IV*c* it is seen that the channel discrepancies are significantly greater for the old group for selection and random errors but that the discrepancy does not rise significantly with age for transposition errors. These results are consistent with the observations in Experiment I in the case of transposition and random errors but selection errors appear to behave quite differently in this experiment.

It can be seen from Table III that the channel discrepancies are greater at 30 per min. than 90 per min. in all cases. Although this speed difference reaches significance in one case only, the direction of the difference indicates that the longer the digits are held in store the more liable are errors to occur. This result is consistent with the idea that all types of error reflect faults in a storage system and not in a perceptual system.

Thus the overall results of Experiment II show the discrepancies between Channels 1 and 2 to be greatly reduced when recall instructions are given after rather than before presentation. This finding suggests that the larger discrepancies in Experiment I were caused by attentional factors and that random errors in particular were caused by a breakdown in perception. The error analysis in Experiment II, however, suggests on the contrary that all types of error are largely errors of storage. The fact that selection errors are greater on Channel 1 in the second experiment reduces the overall channel discrepancy.

Table III shows that the error patterns are broadly similar for the two speeds of presentation. Thus, to facilitate comparison between Experiments I and II, the error scores for the two speeds were averaged and superimposed on Figure 3. This figure shows that the pattern of transposition and random errors is very similar in both experiments. Transposition errors do not increase markedly with age in either channel. Also there is a marked channel discrepancy for these errors and this discrepancy does not increase with age. Random errors do increase with age especially on Channel 2. There is again a well-defined difference between the channels and in this case the discrepancies do increase with age. These similarities in the patterns of errors between the two experiments and the fact that no perceptual advantage to Channel 1 was operating in Experiment II lead to the conclusion that both these types of error are errors of memory. The different pattern of selection errors in the two experiments may mean that these errors arose from different stages of the memory process in each experiment but a simpler explanation is that the perceptual advantage to Channel 1 in the first experiment kept the incidence of selection errors down in that channel. Thus it is suggested that while selection errors are essentially errors of retrieval, perceptual factors have more effect on them than on the other two types of error.

So it appears that in Experiment I the first channel to be reproduced was better than the second partially because of greater attention to that channel but largely because of a greater number of storage errors in the second channel. Further that the greater channel discrepancies shown by older subjects for random errors in both experiments and selection errors in Experiment II are largely due to increased storage difficulties with age.

Two other studies should be mentioned in this discussion. Broadbent (1957) presented subjects with dichotic digits and gave instructions as to which half-set to recall first after presentation. He found that under those conditions, subjects' performance improved with practice and also that while the second half-set was recalled less well than the first at an early stage of practice, this channel discrepancy declined practically to zero later in practice. In terms of his information flow model Broadbent maintains that the first half-set to pass through the p-system has an advantage over subsequent material. He explains the improvement in performance and the decline in Channel discrepancy with practice as being due to a switch in strategy from holding both half-sets of digits in a pre-perceptual store (the s-system) until recall instructions were given, to a strategy of passing one half-set immediately to the p-system. Using the former strategy when the subject knew the recall order he could pass the required half-set through the p-system first, thereby giving Channel

1 an advantage over Channel 2 and giving rise to a channel discrepancy. Using the latter strategy, one channel would still have an advantage on each occasion but on half the trials it would be Channel 2—thus the channel discrepancy would disappear.

It might be thought that a case could be made for the larger channel discrepancy shown by old subjects in Experiment II being due to slower switching to the more efficient strategy. This explanation becomes highly improbable, however, when it is remembered that the pairs of digits in one case arrived every 2 sec.—the possibility that digits from both channels or even from one channel are not perceived until after the complete presentation is quite contrary to subjective experience on the task. Instead it seems certain that both digits in a simultaneous pair are perceived and then stored—at the speeds used in these experiments at least. It is possible that an analysis of the types of errors made by Broadbent's subjects might point to a different explanation of his result.

More sympathy is felt for the account of dichotic listening put forward by Yntema and Trask (1963). They assume that both members of a pair of items are perceived and stored in memory at the time of presentation, that the items are "tagged" in various ways (e.g. position in series, which ear they arrived at, etc.) and that at recall a processor mechanism retrieves the relevant items according to which tags it is programmed to consider. Thus recall is viewed as a search through memory while errors could arise through fading of the various tags either by simple decay over time or by interference from other items. It is quite plausible that how firmly the tag is established initially depends to some extent upon the perceived intensity of the item. Broadbent and Gregory (1963) showed that when attention was diverted from a stimulus source, items from that source were attenuated but not blocked. Thus it could be speculated that in Experiment I, Channel 2 items had a lowered perceived intensity and were thus less firmly tagged than Channel 1 items. Also that Channel 1 items in Experiment II (which were attended to half the time) were similarly less firmly tagged than Channel 1 items in Experiment I (which were attended to all the time). The channel discrepancies in Experiment II make it necessary to postulate further that the tags fade with delayed recall and also that they fade more quickly in older subjects due again either to increased decay over time or to increased liability to interference. The fact that the different types of error show different patterns suggests that the tags fade at different rates—e.g. that the tags representing serial position fade at a different rate from those representing ear of arrival.

While these results and conclusions agree with those of Inglis and Caird to the extent that there are significant age decrements in the ability to reproduce dichotic digits and that these decrements appear to be largely due to increased difficulties of storage, they disagree with them on some points of their explanation. The major difference in the results is that in these present experiments an age decrement was found in the first as well as the second channel to be reproduced. This difference may have been caused by the altered instructions or possibly by the different criterion on which groups were matched (the subjects in these experiments were matched on verbal intelligence while Inglis's groups were matched for orthodox digit-span performance). These present results do not however contradict Inglis and Caird's main conclusion that their results show a deterioration with age in some short-term storage process. They suggest rather that the first half-set reproduced has also been stored in the same system and is thus also liable to storage losses which increase with age.

In view of Welford's suggestion that a decline in the efficiency of short-term memory with age may underlie decrements in learning ability and other intellectual functions it seems highly important to understand in what ways its efficiency declines. From this understanding, conditions which are optimal for older persons' learning and intellectual functioning can be devised. Inglis and Caird threw some light on the problem by using dichotic stimulation and these present experiments have attempted to add to their work by employing different experimental conditions and by analysing errors into different types. Finally, it seems certain that there are complex interactions between such factors as perceived intensity of the stimulus, nature of the stimulus and the age of the subject. Further research must attempt to specify how these and other factors interact to give rise to deficits in short-term memory with increasing age.

SUMMARY

The experiment of Inglis and Caird (1963) was repeated with certain modifications. When subjects aged 20–80 were presented with six digits, two at a time, one to one ear and one to the other at two speeds—30 pairs a min. and 90 pairs a min.—it was found:

(*a*) When instructions as to which ear to recall first were given beforehand (Experiment I):

(1) Performance on both Channel 1 (the ear recalled first) and Channel 2 (the ear recalled second) declined with age.

(2) At 30 per min. the discrepancy between Channels 1 and 2 became significantly greater with age. This was not so at the faster speed.

(3) In an attempt to determine which part of the process was giving rise to errors in reproduction, the errors were classified into three main types: selection, transposition, and random. Selection errors showed little differences between channels and did not appear to rise substantially with age; it was concluded that they occurred during input. Transposition errors were consistently greater in Channel 2 and so were held to be a function of time spent in storage. As the channel discrepancy for this type of error did not increase with age it was concluded that storage difficulties did not increase with age. The channel discrepancy for random errors did increase markedly with age however. They were judged to be errors due to a decreased effective signal strength on Channel 2 as attention was diverted from that channel. Further, it was concluded that age differences on the dichotic listening experiment were most likely to be caused by increased perceptual difficulties rather than increased memory difficulties.

(*b*) When instructions as to which ear to recall first were given after presentation (Experiment II) thereby equalizing attention between channels:

(1) It was confirmed from Experiment I that there was a decline with age in *both* channels.

(2) The channel discrepancies while still significant, were much less than in Experiment I.

(3) Transposition errors were significantly greater in Channel 2, thus confirming that they were errors due to storage.

(4) The channel discrepancy for transposition errors did not increase significantly with age. This confirmed the observation in Experiment I and the conclusion that this type of storage difficulty does not increase with age.

(5) Random errors were greater in Channel 2 and were thus concluded to be errors due to storage rather than input.

(6) Channel discrepancies for random errors did increase significantly with age—thus this type of storage difficulty apparently does increase with age.

(7) Selection errors were significantly greater on Channel 1 which suggested that they were errors of retrieval rather than input. Channel discrepancies for this type of error also increased significantly with age.

(8) While the lower overall channel discrepancies suggested at first that "input" or "attention" factors played a large part in Experiment I, the analysis of types of error showed that, on the contrary, storage factors were more important.

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SIMILARITY AND INTRUSIONS IN SHORT-TERM MEMORY FOR CONSONANT-VOWEL DIGRAMS

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Subjects listened to lists of six consonant-vowel digrams presented at the rate of 0·8 sec./digram and copied them as they were being presented. Immediately after finishing copying the list, they attempted ordered recall of the six digrams. The digrams in each list were chosen from a population of eight digrams consisting of all digrams that can be constructed from the consonants "f" and "n," the vowels "ā" and "ō," and the two orders "CV" and "VC." Intrusions tended to be similar to the presented digram, and the frequency of an intrusion was a monotonic increasing function of degree of similarity to the presented digram. The ordering of intrusion frequency for each similarity type was from greatest to least: +−+ (same consonant, different vowel, same order), ++−, −++, −+−, +−−, −−+, −−−. The findings indicate that forgetting is not all-or-none, that digrams are coded in terms of phonemes, and that initial vs. terminal position is a distinctive feature of consonants, but not vowels, in short-term memory.

Conrad (1964) and Wickelgren (1965) have shown that intrusion errors in short-term recall of letters and digits tend to have a vowel or consonant phoneme in common with the correct letter or digit. Wickelgren's study suggested that vowel similarities were more important than consonant similarities, but the study was far from systematic with respect to this question. Letters and digits are an unsystematic population of combinations of phonemes. The number of different vowel phonemes is smaller than the number of different consonant phonemes in the letters and digits, and a few vowel phonemes (ē, ĕ, ā, ī, ōō) are found in many letters and digits, while no consonant is found in a similarly large number of letters and digits.

There was some indication in Wickelgren's study that common-consonant intrusions had to have the common consonant in the same serial position in the sequence of phonemes composing an item, while common-vowel intrusions were less dependent on the relative position of the vowel. Thus, "g" (/jē/) and "j" (/jā/) were frequently confused in recall, as were "s" (/ĕs/) and "x" (/ĕks/), but "k" (/kā/) and "x" (/ĕks/) were not. At the same time "h" (/āʃ̂/) and "j" (/jā/) were confused even more often than "j" (/jā/) and "k" (/kā/), etc. However, there are extremely few comparisons of this kind that can be made with letters and digits, and it takes a huge sample to get enough cases of these types of errors to make a meaningful comparison.

Wickelgren's study, at best, is only suggestive regarding the relative importance of consonant similarity, vowel similarity, and position similarity in producing intrusions in short-term recall. Furthermore, there are no cases where one letter consists of the same two phonemes as another letter, but in the opposite order. Would this sort of similarity produce a very high probability of confusion between the two letters in short-term recall? How would the frequency of this sort of intrusion compare to the frequency of intrusions that had a common consonant or vowel in a common position, and how would each of these frequencies compare to the frequency of intrusions having either a consonant or a vowel in common with the correct item, but in a different position? Finally, how would each of these three types of intrusion frequencies compare with the frequency of intrusions that have no phoneme in common with the correct item? Previous studies only indicate that intrusions tend to be phonemically similar to the correct item; they do not evaluate the relative importance of consonant, vowel, and position similarity in producing intrusion errors. This is the purpose of the present study.

METHOD

Procedure

One sec. after a "ready" signal, subjects listened to a list of six consonant vowel digrams and copied them as they were being presented. Immediately after finishing copying, they covered what they had copied and attempted ordered recall of the list (by filling-in six boxes). Presentation was at the rate of 0·8 sec./digram, and about 23 sec. was allowed for recall, followed immediately by the "ready" signal for the next trial. The entire experiment was recorded on tape and lasted about 1 hr.

Design

The digrams were the eight possible digrams that can be constructed out of the two consonants "f" and "n," the two vowels "ā" and "ō," and the two orders "CV" and "VC" (i.e. fā, āf, fō, ōf, nā, ān, nō, ōn). Subjects knew what the item population was. One hundred lists were used in the experiment. There were two types of lists, lists consisting of a random arrangement of six different digrams and lists consisting of a random arrangement of five different digrams one of which was used twice (but not adjacent to each other). Four lists in every block of five lists were of the former type, and one list was of the latter type. The only reason for having some lists with repeated items was to prevent subjects from ruling out a digram in recall because they had used the digram already in recall of the list. Cognitive strategies like this would simply add variance to the intrusion data or reduce the number of intrusions by increasing the number of omissions.

Subjects

Subjects were 13 M.I.T. undergraduates taking psychology courses; they participated in the experiment as part of their course requirements.

RESULTS

The purpose of having subjects copy the digrams during presentation was to separate the errors in perceptual recognition from the errors in recall. Only the digrams copied correctly were scored for correct ordered recall. The per cent. correct recall after correct copying was virtually the same for the eight different digrams, varying from 60 per cent for "nā" to 70 per cent. for "ōf." The Spearman rank order correlation between per cent. correct recall of a digram and the number of times that digram was given as an intrusion was 0·88 ($p < 0{\cdot}01$). This indicates that the small differences in frequency of correct recall were partly or completely the result of different response biases for different digrams.

Table I reports the frequency of correct ordered recall and the frequency of each type of error for each of the eight digrams. The data in Table I are totals for all subjects and all lists. What defines a type of error in Table I is its similarity to the presented digram. A three-dimensional vector is used to describe the type of similarity of an intrusion to the presented digram. The first dimension is "+" if the consonant in the intrusion is the same as the consonant in the presented digram and "−" if it is different. The second dimension is "+" if the vowel in the intrusion is the same as the vowel in the presented digram and "−" if it is different. The third dimension is "+" if the consonant-vowel order is the same in the intrusion as in the presented digram and "−" if it is different.

TABLE I

FREQUENCY OF CORRECT AND INCORRECT ORDERED RECALL

Presented digram	*Correct digram*	*Intrusion similarity types*							*Omissions*	*Copied incorrectly*
	+++	++−	+−+	−++	+−−	−+−	−−+	−−−		
fā	642 (67%)	āf 38	fō 51	nā 28	ōf 31	ān 23	nō 19	ōn 17	105	73
āf	567 (63%)	fā 62	ōf 79	ān 31	fō 20	nā 26	ōn 14	nō 13	92	123
fō	622 (62%)	ōf 75	fā 86	nō 35	āf 13	ōn 40	nā 15	ān 16	107	83
ōf	587 (70%)	fō 35	āf 42	ōn 23	fā 16	nō 27	ān 18	nā 7	85	57
nā	531 (60%)	ān 59	nō 43	fā 44	ōn 31	āf 23	fō 17	ōf 34	111	108
ān	586 (66%)	nā 32	ōn 44	āf 26	nō 18	fā 40	ōf 27	fō 19	100	83
nō	484 (61%)	ōn 51	nā 25	fō 39	ān 26	ōf 43	fā 23	āf 14	93	73
ōn	571 (67%)	nō 41	ān 34	ōf 38	nā 14	fō 29	āf 9	fā 17	99	58
Total	4590 (64%)	393	404	264	169	251	142	137	792	658

The ordering of total intrusion frequency for each similarity type was from greatest to least: +−+ (same consonant, different vowel, same order), ++− (same consonant, same vowel, different order), −++ (different consonant, same vowel, same order), −+− (different consonant, same vowel, different order), +−− (same consonant, different vowel, different order), −−+ (different consonant, different vowel, same order), −−− (different consonant, different vowel, different order). We can determine if the frequency of one type of intrusion differs significantly from the frequency of another type of intrusion by applying the Wilcoxon Matched-Pairs Signed-Ranks test to the differences between these frequencies for the 13 subjects ($N = 13$ in each comparison). Every similarity type of intrusion can occur in response to every type of presented digram, and every similarity type of intrusion includes each of the eight digrams once in response to different presented digrams. Therefore, the totals for each similarity type of intrusion are not affected by differences in the number of times each digram was presented and copied correctly, nor are the totals affected by differences in response biases for different digrams.

TABLE II

2 × 2 CONTINGENCY TABLE FOR "ōf"–"nō"

		Recalled		
		nō	Not nō	
Presented	ōf	27	141	168
	Not ōf	169	1423	1592
		196	1564	1760

$\chi^2 = 4{\cdot}04$

By a two-tailed Wilcoxon test, intrusions of type +−+ did not occur significantly more often than ++−, but each occurred significantly more often than each of the other five types of intrusion (each $p < 0{\cdot}01$, except (++−) > (−++) at the 0·02 level). Intrusion −++ did not occur significantly more often than −+−, but each occurred significantly more often than +−−, −−+, and −−− (each $p < 0{\cdot}01$). Intrusion +−− occurred more often than −−+ and −−−, but the differences failed to reach statistical significance. Intrusions −−+ and −−− were virtually identical in frequency.

Since the number of times each digram was presented and copied correctly was not equal for all digrams and the "response biases" were not equal for all digrams, it is not possible to draw valid conclusions by simply comparing the raw frequencies in different cells of the error matrix in Table I. However, we can compute a χ^2 value for each entry in the table which represents the degree to which the entry exceeds (+) or falls short of (−) the frequency expected by chance. The χ^2 value for each entry is that associated with the 2 × 2 contingency table formed as indicated in Table II for the entry "ōf"–"nō."

Table III reports the χ^2 value associated with each entry in the error matrix, computed as in Table II, but excluding the frequencies of correct recall. If frequencies of correct recall are not excluded, all one gets is a large number of negative χ^2 which merely means that subjects tended to recall correctly. We are interested in whether some types of errors are more frequent than others.

TABLE III

χ^2 VALUES FOR EACH INTRUSION IN TABLE I

Presented digram	*Intrusion similarity types*						
	++−	+−+	−++	+−−	−+−	−−+	−−−
fā	āf 21·1	fō 34·7	nā 7·5	ōf −1·8	ān −0·4	nō −0·7	ōn −3·5
āf	fā 15·9	ōf 34·1	ān 0·1	fō −3·4	nā 1·6	ōn −11·3	nō −9·1
fō	ōf 14·2	fā 48·9	nō 0·5	āf −8·1	ōn 0·8	nā −3·5	ān −11·0
ōf	fō 13·1	āf 51·4	ōn 0·1	fā −5·8	nō 4·0	ān −0·1	nā −3·7
nā	ān 37·6	nō 9·9	fā 0·3	ōn 0·0	āf 0·0	fō −6·9	ōf −4·5
ān	nā 14·7	ōn 15·8	āf 2·5	nō −1·1	fā 1·3	ōf −4·2	fō −1·3
nō	ōn 24·8	nā 2·5	fō 7·2	ān 0·0	ōf 0·7	fā −6·1	āf −2·4
ōn	nō 25·4	ān 8·6	ōf 0·6	nā 0·4	fō 2·7	āf −4·1	fā −6·8
Total ..	155·5	175·9	10·1	−13·5	5·0	−31·6	−35·8

The frequencies of any two types of intrusions can now be compared by applying the Wilcoxon Matched-Pairs Signed-Ranks test to the eight difference scores between the χ^2 values for the two types of intrusions. All of the comparisons that were significant by the differences for each subject totalled over the eight presented digrams were significant by the differences for each presented digram totalled over the 13 subjects. Furthermore, by this latter test, +−− occurred significantly more often than −−− ($p < 0.05$, two-tailed). Confirming all of the significant differences by the latter test was possible only because the relative frequency of each type of intrusion was extremely consistent for different presented digrams.

DISCUSSION

Not only are intrusions likely to have some phoneme in common with the presented item, but the probability of an intrusion appears to be a monotonic increasing function of the degree of similarity to the presented item. Similarity in this study is in terms of same vs. different consonant or vowel phonemes and same vs. different consonant-vowel order, and each of these three dimensions of similarity is clearly an important determinant of the probability of an intrusion.

However, there is only an insignificant tendency for intrusions with consonant and vowel both different from the presented digram to have the same consonant-vowel order. Also, intrusions with the same vowel as the presented digram, but a different consonant, are only insignificantly more likely to have the same consonant-vowel order than the different order. However, intrusions with the same consonant as the presented digram are overwhelmingly more likely to have the same consonant-vowel order than to have the different order.

The findings of the present experiment indicate that the degradation of a memory trace, whether by decay or interference, is not all-or-none. There are intermediate states where only part of the trace is above the level of competing traces, and what is recalled is similar to what was presented by virtue of the parts of the trace that are above the level of competing traces. Thus, differences in frequency for different types of intrusions provide information on the "parts" of the memory trace. It would be very difficult to account for the present findings without assuming that the trace for a consonant-vowel digram is composed of at least semi-independent traces for the consonant, the vowel, and the consonant-vowel order. However, the results suggest that, while the nature of the consonant and the nature of the vowel are always important dimensions of similarity, the relative position is important only for consonants and only when the trace for the correct consonant is above the level of competing traces. It would describe the data more accurately to say that there are two dimensions of similarity: (1) consonant similarity, with three values (same consonant in the same position, same consonant in the different position, different consonant) and (2) vowel similarity, with only two values (same vowel, different vowel). The trace for the consonant can be lost in two stages; the trace for the vowel is lost in only one stage.

From a structural point of view, this difference between consonants and vowels might result from the following. Suppose there is only one internal representative of a vowel in short-term memory, but there are two internal representatives of a consonant, one for the initial consonant phoneme and one for the terminal consonant phoneme. These two representatives of the "same" consonant must either be overlapping or have strong long-term associations between them, because intrusions with the same consonant in the different position are more likely to occur than intrusions with the different consonant, other factors being equal (i.e. +−− intrusions occur more often than −−− intrusions, significantly more often in one of the two analyses, and ++− intrusions occur significantly more often than −+− intrusions). At the same time the internal representative of an initial consonant must be somewhat different from the internal representative of a terminal consonant because intrusions with the same consonant in the same position are much more likely to occur than intrusions with the same consonant in the different position (i.e. +−+ intrusions occur significantly more often than +−− intrusions and correct responses, +++, occur significantly more often than ++− intrusions). When the trace for the consonant has been degraded to the point where the incorrect consonant is recalled, the trace for consonant-vowel order appears also to have been lost (i.e. −−+ intrusions are not significantly more frequent than −−− intrusions and −++ intrusions are not significantly more frequent than −+− intrusions). It fits the intrusion data of the present study very well to assume that the internal representative of a consonant-vowel digram is composed of an internal representative of the consonant phoneme and an internal representative of the vowel phoneme, with initial or terminal position being a feature of the consonant, but not the vowel.

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SHORTER ARTICLES AND NOTES

LENGTH OF EXPERIMENTAL SESSION AS A PARAMETER OF RESPONSE RATE UNDER A NON-DISCRIMINATED AVOIDANCE SCHEDULE

BY

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Following the stabilization of response rate under an avoidance schedule which was defined by two temporal parameters, the shock-shock interval and the interval by which each response postponed the onset of shock, the length of the experimental session was changed. It was found that after the subjects had been exposed to a longer session of avoidance schedule, their rates of response were considerably increased without a corresponding reduction in the number of shocks received.

In recent years considerable use has been made of an avoidance training technique in which the performance of the response functions to postpone the onset of an aversive stimulus, usually shock, by a fixed period. In the absence of the required response the aversive stimulus is programmed to occur at regular intervals. Experiments by Sidman (1953) have shown that the critical independent variable controlling the rate of avoidance response, is the shock postponement interval (R*S). All other things being equal, the rat in the lever pressing situation will respond at a rate which is inversely related to the R*S interval, low intervals generating high response rates and high intervals generating low response rates. However, under very low values of R*S, the response rate may break down altogether. The animal then receives shock at the rate determined by the shock-shock interval parameter.

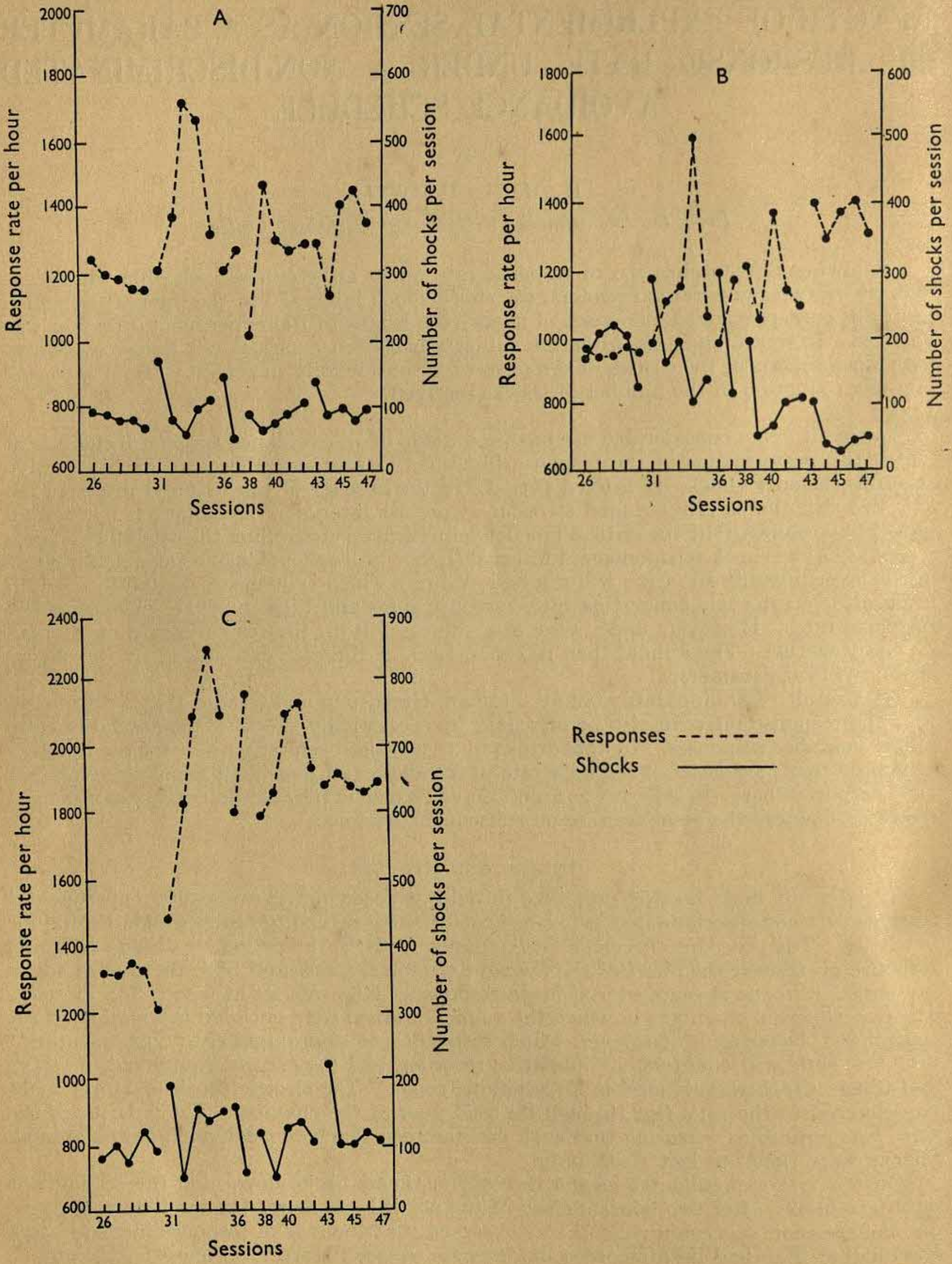
As a result of an apparatus failure, Sidman, Herrnstein and Conrad (1957) discovered that the response rate can also be increased by occasionally shocking the animal in spite of its avoidance responses. An apparatus failure has also been responsible for the isolation of yet another parameter of response rate in the shock-postponement avoidance situation and is reported here. Briefly, it was found that a change in the duration of an experimental session influences the response rate on subsequent sessions.

METHOD AND RESULTS

Three adult male hooded rats were initially exposed for 30 successive experimental sessions to a non-discriminated avoidance conditioning procedure, each session lasting for one hour. The procedure involved two parameters: R*S, the period by which each lever response postponed the onset of the aversive stimulus, shock and S*S, the rate of shock presentation in the absence of avoidance responses. R*S was set at 9 sec., S*S at 5 sec. The experimental chambers in which the animals worked were enclosed in sound resistant containers. Depression of a lever, which responded to approximately 7 gm. downward pressure, activated counters, a cumulative recorder, and the control equipment of relays and timers which were housed in an adjoining room. The electric shock (0·25ma) could be delivered to the rat's feet through the grid floor of the chamber. The polarity of the grids was scrambled, ensuring that each programmed shock was delivered to the animal. Shocks were timed to last 1 sec. only.

On the 31st experimental session the session timers broke down and the session was only terminated after two hours instead of the normal one hour. On the following day a one hour session was again run. It was observed that the rate of response increased to a level higher than had been recorded on days 26–30 (see Figure 1). Figure 1 A, B and C, gives the number of responses per hour for each of the three animals during sessions 26–47 and also the number of shocks received during each session. The data shows that on the 31st session the response rates for animals A and B did not differ substantially from those of the previous five sessions, whereas rat C increased its rate of response. On subsequent sessions (32–34) the rates increased to approximately twice their previous

FIGURE 1



Response rates per hour and total number of shocks received by each subject (A, B, C). The break between records indicates a change in the experimental procedure from one hour sessions to a two hour session (sessions 31, 36, 38, 43).

level for each animal. It seems reasonable to attribute this change in rate to the session time on the previous day. Since rats A and B showed a decline in response rate on session 35 (although their rates were still higher than during sessions 26–30), a further two hour session was given (session 36) and again all three animals demonstrated an increase in response rate on the following day (session 37). Session 38 was again a two hour session, and observations of the response rate were obtained for a further four sessions. The effects already noticed for session 32–35 were repeated, the form of the function being the same. The cycle of one two hour session and four one hour ones was repeated once more with comparable effects. At this point the experiment had to be terminated.

Certain features of the result call for special attention. First, the rates per hour for the four two-hour sessions do not appear to be different from the rates observed in the one-hour sessions between session 32–37. The exception to this is animal C. However, it was found that this was largely due to a fall in rate during the second hour of a two-hour session.

The solid lines in Figure 1 represent the total number of shocks received during the individual sessions. As expected, the number of shocks in the 2-hour sessions is consistently greater than during the 1-hour sessions. However, despite the high response rates observed in the 1-hour sessions following session 31 the number of shocks received by animals A and C during these sessions do not appear to be any *lower* than during sessions 26–30. Animal B, however, which received many more shocks during sessions 26–30 than did the other two animals, reduced the number of shocks received in the subsequent one-hour sessions. The results indicate that the absolute values of the response rates and the number of shocks received are not closely related. Large increases in response rate did not produce a correspondingly sizeable decline in shocks received and large decrease in response rates did not substantially increase the number of shocks. The record of Animal B, Figure 1, illustrates this particularly clearly. Taking only the 1-hour session days, we see that a three or four fold increase in response rate produced only small decreases in the number of shocks received.

In evaluating the results, it is important to establish that the rates of response observed prior to the introduction of the experimental variable, namely session-length, satisfies a criterion of steady state (Sidman 1961). The rates of response during session 26–30 show very little day to day fluctuations. Furthermore, their variance is small compared to the variance of scores following the first two-hour session. One may, therefore, conclude that the change in the response rate following session 31 is attributable to the manipulation of session-lengths so that *session-length* qualifies as a parameter of avoidance response performance under the schedule contingencies described.

The work was supported by funds made available by the Medical Research Council.

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POSITION REVERSAL TRAINING IN THE VIRGINIA OPOSSUM: EVIDENCE FOR THE ACQUISITION OF A LEARNING SET

BY

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To obtain an estimate of the learning ability of opossums, five tame ones were trained on a series of position reversals to a criterion. With 4 trials per day there was no consistent improvement over a series of 15 reversals. When each daily session was increased to as many trials as needed to reach criterion, errors dropped sharply, indicating the formation of a position learning set. Upon return to 4 trials per day, 3 of 4 animals continued to perform with few errors, showing transfer of learning set. Opossums do more poorly than rats on this task, which is in agreement with the relative phyletic level of the two species.

INTRODUCTION

According to Herrick (1933), a prominent feature of the evolutionary transition between reptiles and mammals is the appearance of neopallium in primitive mammals such as the opossum; increases in the relative size and anatomical complexity of this structure culminate in the primate neo-cortex. Young (1962) points out that the Virginia opossum (Didelphis virginiana) has retained morphological features of the earliest known marsupials and bears strong similarities to prototypic placental mammals. Although this status has led to numerous anatomical and physiological investigations of the opossum, there have been very few behavioural studies (James, 1955; 1958; 1959; 1960). However, the availability of tame opossums (Friedman, 1964) has made this species a more attractive subject. As an initial study it appeared desirable to obtain an estimate of the learning ability of the opossum by comparing it to the rat. A two-choice position discrimination was decided upon since it provides a measure which is minimally dependent upon perceptual-motor capacities and opossums have been shown to perform well on a position learning task (James, 1958). In addition, Harlow (1959) has suggested that a measure of the ability to improve over a series of problems, i.e. to form a learning set, is closely related to cortical complexity and is particularly suitable for interspecies comparison. An experiment by Dufort, Guttman and Kimble (1954), in which rats were shown to rapidly acquire a position learning set, is the basis for the present experiment, which uses equivalent apparatus and procedure to investigate position learning in the opossum. An estimate of the opossum's ability to perform in a relatively complex learning situation would be useful for further studies on this species and, in view of the opossum's phyletic status, a demonstration of its ability to acquire a learning-set would offer extended perspective on the generality of this phenomena.

METHOD

Subjects. Five adult Virginia opossums which had been raised in the laboratory proved sufficiently tractable to allow ease of handling and a rapid habituation to the experimental apparatus. Rearing and taming procedures have been described by Friedman (1964). All five were from the same litter, two being male and three female. They were maintained on a 23 hr. feeding schedule with dry Purina Dog Chow given for one hour after completion of each daily experimental session. One male became ill and was terminated after having completed the 25th reversal; up to this point its performance had not differed appreciably from that of the other animals.

Apparatus. A two-choice discrimination box similar to that used by Dufort *et al.* (1954) and described by Grice (1948) was employed. This apparatus consists of a starting box, choice compartment, and two food boxes located approximately 60 cm. from the entrance to the choice compartment. A panel 15 cm. wide separated the doors to the food boxes; each door was distinctively marked using black and white stripes, those on the left running vertically and those on the right, horizontally. A sliding panel was so situated that it could be lowered to cover both doors. With this panel raised the doors were illuminated by two 15 w. incandescant bulbs counted at either side of the starting box door. The starting box was illuminated by a 7½ w. bulb. The experimental room was kept relatively dark and quiet during training sessions.

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Procedure. Each opossum was given several days of pre-training in the apparatus; during this time it was trained to touch its nose to a goal box door, upon which the door was opened allowing access to food (approximately 12 gm. of ground horse meat). Raising of the starting gate was initiated by the opossum nosing the gate which it generally did within 10 to 15 sec. after being placed in the starting box. On the last day of pre-training, five trials were given to determine each animal's position preference; reinforcement was given on each of the trails. On the following day, a series of right-left position discriminations was begun. On the first problem, the correct side was opposite to that initially preferred; after the animal had taken food from the correct food box or nosed the incorrect door (each food box contained meat on all trials), the sliding panel was lowered marking the end of the trial; correction was not permitted. When the problem was learned to a criterion of 11 correct responses out of 12 with the last 8 responses being correct, the previously incorrect side became correct for the next series of trials. The experiment was run daily, with no interruptions, and was divided into four parts as follows:

I. Reversals 0 through 15. Four trials were given each day; if criterion was reached in the middle of the session the remaining trials were run and the problem reversed on the first trial of the following day. Each reinforcement consisted of approximately 12 gm. of ground horse meat. This procedure is essentially the same as that used by Dufort *et al.* (1954).

II. Reversals 16 through 25. Each daily session was run with as many trials as were necessary for the animal to reach criterion. A new reversal was begun with the first trial of the day. The size of the reinforcer was reduced to one-half that used in Part I (i.e. from 12 gm. to 6 gm.).

III. Reversals 26 through 30. Conditions were the same as those of Part II with the exception that at the start of each daily session, the opossum was run until it made four reinforced responses to the side which had been correct on the previous day. On the next trial following the fourth reinforced response the correct side was reversed and training continued until criterion was reached.

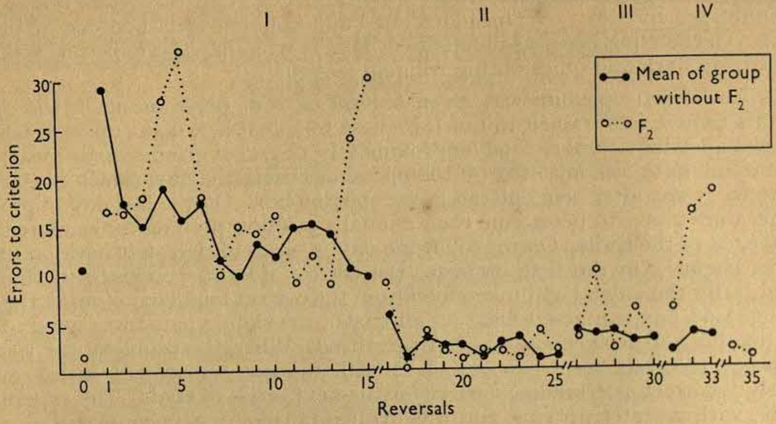
IV. Reversals 31 through 33. Conditions were the same as those of Part I.

Results

The number of errors to reach criterion for each of the reversals are shown in Figure 1; the scores for one female (F_2) are shown separately from the mean scores of the group. Table I gives the mean errors and trials per reversal for individual animals in each part of the experiment. The initial discrimination is designated R_0. There is a marked increase in errors for R_1 compared to R_0 for all animals; indicating that the position preference acquired during R_0 was difficult to extinguish. The decline in mean errors from R_1 to R_8 (i.e. to approximately the level of R_0) was also seen for the individuals in the main group. With the last seven reversals of Part I there was no further improvement and none of them was able to perform consistently better than its R_0 score. The very rapid learning of R_0 by F_2 is in sharp contrast to the relatively poor performance of this animal during Part I.

The change to Part II (i.e. from four trials a day to as many as were required for reaching criterion) resulted in a rapid decrease in both errors and number of trials to reach criterion for all animals. The performance of each one in the main group on R_{16} was superior to its R_0 score. Low error performance was maintained throughout Part III except by F_2 with 10 errors on R_{27}; the performance of the other animals closely matched the mean of the group. During Part IV (four trials a day), three of the four continued to perform with few errors. The performance of F_2, which in Part I was generally inferior to that of the other animals, showed a sharp increase in errors in Part IV and for this animal performance in Part IV was no better than in Part I. When two additional reversals (R_{34} and R_{35}) were given F_2 under the conditions of Part II, the number of errors dropped to 2 and 1, respectively. F_2 also did considerably poorer than the others with respect to the number of trials to reach criterion; for this opossum errors were not concentrated at the beginning of each reversal as they were for the others.

FIGURE 1



Errors to criterion for each reversal. The mean of the group without F_2 is shown by the solid line; the errors of F_2 are shown by the dotted line.

Discussion

In the experiment on rats by Dufort *et al.* (1954), R_0 was learned with a mean of four errors and R_1 with a little over nine errors. All rats performed with only one error by R_8. In contrast, the opossums clearly failed to acquire a position learning set under these conditions. The general decline in errors in the first half of Part I, which stops at the level of R_0, suggests that the *disruptive* effect of the preceding training decreased during this period. However, there is no evidence in Part I of a *facilitation* of learning yielding an improvement over the R_0 performance (such as seen with rats) which would have indicated the formation of a learning set. The rapid reduction in errors accompanying the change to Part II, however, would indicate that the nearly 24 hours' separation between blocks of four trials in Part I was an important factor. It appears that while spaced trials permitted the learning of individual problems, inter-problem learning was impeded. The use of massed trials markedly aided both types of learning. The importance of the spacing of trials is much more striking here than it is in the case of rats and position reversal (Stretch, McGonigle and Morton, 1964).

TABLE I

Mean Errors (E) and Trials (T) to Criterion for Each Subject

Animal		*Part I*		*Part II*	*Part III*	*Part IV*
		R_0	R_1—R_{15}	R_{16}—R_{25}	R_{26}—R_{30}	R_{31}—R_{33}
M_1	E	16	15·1	2·7	3·8	2·0
	T	41	34·9	15·1	14·4	17·3
M_2	E	12	17·3	2·3	—	—
	T	32	35·1	13·8	—	—
F_1	E	9	13·9	2·3	3·6	3·3
	T	28	33·3	15·5	15·0	21·7
F_3	E	6	13·3	2·6	2·8	3·3
	T	29	32·0	15·8	15·4	20·0
F_2	E	1	16·8	3·1	4·8	13·3
	T	13	48·2	15·1	16·0	38·7

The conditions of Part II differed from those of Part I not only in the number of trials per day but also in the nature of the discrimination task; the first trial of each daily session began a new problem. To test the relative importance of these two factors, Part III was run with each reversal placed within the course of the daily session. Though there resulted a slight increase in errors, it is clear that even under these conditions the opossums learned to reverse rapidly with few errors.

Part IV (four trials a day) was carried out to determine whether or not the learning set acquired in Part II would transfer to a condition which was not initially sufficient for the establishment of this set. For three of the four opossums, transfer did take place as shown by the low error scores; the one with the poorest performance in the previous parts of the experiment (F_2) was unable to show transfer in Part IV. For F_2, a return to massed trials (R_{34} and R_{35}) resulted in rapid reversal which confirmed that the distribution of trials was the limiting factor. The performance of three of the four opossums provides evidence that learning to learn may transfer in much the same manner as other forms of learning.

It is possible that the rapid reduction in errors in Part II indicates that some learning set formation took place during Part I. Although the design permits no direct test of this, once they had learned to reverse with few errors, a return to four trials a day did not hinder continued low-error performance. Another problem is that the performance in Part IV (R_{31} through R_{33}) might not have been different had all 30 of the preceding reversals been run using four trials per day. The failure of the opossums to show any consistent improvement over R_0 scores in Part I suggests, however, that little or no decrease in errors would have occurred by R_{30} under this condition.

Although both the opossums in the present study and the rats used by Dufort *et al.* were run with 23 hour food deprivation, drive levels were probably not equivalent. Opossums require relatively less food than does the smaller and more active rat. The deprivation schedule, therefore, was probably less severe for the opossums and this may have contributed to their inferior performance.

The relative difficulty of the reversal problem for opossums compared to rats permitted an investigation into some of the experimental conditions which affect this type of learning. The major difficulty for the opossum appears to be that long delays between blocks of trials tends to impair learning to learn much more than the learning of individual problems. This is in agreement with Harlow's (1959) finding that interproblem learning differs from and is more difficult than intraproblem learning. When a delay between blocks of trials was eliminated (as in Parts II and III), improvement was rapid and the final performance approximated that of rats. The ability of low grade mental defectives to do as well as rats on a position reversal task in a study by House and Zeaman (1959) may have been similarly facilitated by the use of massed trials. These results suggest that opossums learn best with massed trials and generally perform somewhat more poorly than rats. The superiority of the rat to the opossum, both in ease of learning set formation and in final performance, is in agreement with the relative phyletic level of the two species.

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THE EFFECT OF ANXIETY ON FOOD INTAKE IN THE RAT

BY

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Three levels of anxiety were induced experimentally in rats. With hunger drive held constant, the effects of anxiety on the intake of food which had either a positive or negative incentive value were observed and compared. The results for the intake of food with positive incentive value showed eating to be increased above control level by a low level of anxiety, and slightly decreased by medium and high levels. On the other hand, a direct relationship was observed between level of anxiety and suppression of the intake of food with a negative incentive value. These findings were shown to be inconsistent with previous explanations of similar effects in terms of an interaction between relevant and irrelevant drives. An alternative explanation in terms of incentive was offered.

INTRODUCTION

At the outset, it is necessary to define what is meant by "anxiety" in the present study. It merely refers to that state which exists in an organism in a slowly dissipating manner following the offset of any traumatic experience, more specifically, electro-shock. This is not anxiety as conditioned to a specific stimulus situation. In the past, similar studies have referred to "emotionality" (e.g. Siegel and Siegel, 1949), "emotional state" (Estes, 1944), or have simply spoken of the after-effects of shock, using no label at all (Ducharme and Belanger, 1961). Anxiety, however, is thought to be a relatively pertinent term in that certain of the behavioural consequences of electro-shock are indicative of what has been formerly styled anxiety. These are responses such as hyperactivity, trembling, drooling, excessive respiration and excretion, etc. This type of anxiety state has been viewed by Hull (1943) as an irrelevant drive which interacts with relevant drives (e.g. hunger) to produce overall drive level. Some support has been found for this position. Siegel and Siegel (1949) placed rats that had been water-deprived in a shock-box very different from the drinking situation, and shocked them immediately prior to their normal drinking times. For the next 2 hr. the thirsty, shocked animals were found to drink significantly more than controls who had not experienced shock. Using essentially the same procedure, Amsel and Maltzman (1950) confirmed this finding. In a third study, Siegel and Brantley (1951) established their rats on a food-deprivation schedule, and then, on the test days, shocked them immediately before eating. They found the experimental group to consume significantly more than non-shocked controls. These results would seem to support Hull's theoretical position, but it is not clear just from the increases seen in consummatory responding that anxiety affects the overall drive state of the animal. It is possible that it is influencing the incentive value of the subsequent stimulus conditions. The present study was designed to test this possibility.

METHOD

Subjects

Seventy, experimentally naïve, male, hooded rats of the Sprague-Dawley strain were used. At the onset of experimentation they were all approximately 200 days old, weighing between 163 and 353 gm.

Apparatus

The home cages, constructed of galvanized steel, were 10 in. wide, 13½ in. deep and 10 in. high. The floor, and front hinged door, were of ¼ in. steel mesh. At the front right-hand corner a small upright bar was used to secure the food pot in the same position each day.

The shock box was designed for minimal stimulus generalization to the home cages which were to serve as the testing situation. It measured 9 in. by 9 in. by 3 in. high. The sides were constructed of hardboard and the sliding top of perspex. The floor through which the shock was delivered, was constructed of seven brass rods, ½ in. in diameter, separated by $\frac{7}{10}$ in. These rods were connected to a constant current generator with a chopped current output held at 2·6 milliamps. D.C.

Procedure

1. *Deprivation.* Rats were randomly divided into four groups of 10, three groups of 5 and one group of 15. All animals were established on a 23-hr. food deprivation schedule, being fed for 1 hr. between 11 a.m. and 12 noon daily. This was done until a stable level of food intake was attained for seven consecutive days. For different groups this criterion took between 10 and 14 days to achieve. All the rats had been reared and were fed on a diet of Glaxo 41b with water ad libitum at all times.

2. *Testing.* (*a*) *Positive incentive.* On the test day each of the animals in the three groups of five were placed individually into the shock compartment for the first time. This shock compartment was located at some distance from the home cages. The members of the three groups were given durations of 3, 30 and 300 sec. respectively of 2·6 milliamp. shock through the grid floor. At the termination of shock the rats were immediately transferred back to their home cages, and given one hour's access to their *normal* food diet. Care was taken in shocking them that the termination of shock should coincide with the onset of their normal eating session. The group containing 15 animals served as a control for the above experimental groups. On the test day they were merely subjected to their usual one hour of eating.

(*b*) *Negative incentive.* On the test day the rats in three of the four groups containing 10 animals each received precisely the same shock treatment as that given to the three groups outlined above. The same precautions were taken. The food to which the rats in these three groups had access on the test day was their normal diet adulterated with a 0·2 per cent. mixture of quinine alkaloid. The 10 animals which served as controls for the quinine groups were given the adulterated diet on the test day, but were not subjected to shock.

Whilst the animals in the six shock groups were shocked they displayed hyperactivity, high-pitched vocalization, defaecation and micturation. This, and the considerable difficulty encountered in transferring them back to their home cages gave additional evidence of an aroused state of anxiety. On the test day, food intake in grams was calculated by weighing the unconsumed food every 10 min. for the first half-hour of feeding, and subsequently for the whole hour. This procedure was adhered to for all animals, irrespective of whether or not they had received shock.

RESULTS AND ANALYSIS

The general effects of anxiety on eating under conditions of constant hunger drive (23 hr. deprivation) and varying incentive can be seen in Figure 1. The baseline on the histogram is the mean food intake on the test day for each control group (i.e. that for positive incentive and that for negative). The test day food intake for the experimental groups is expressed as a percentage deviation of this. Results given in the histogram are the mean deviations and ranges, clear columns representing intake under positive incentive conditions and shaded columns intake with negative incentive conditions.

It can be seen from Figure 1 that the effect of 3 sec. shock (low anxiety) was to raise subsequent eating by an average of 80 per cent. above controls where the diet was normal (i.e. had a positive incentive value). The effects of 30 sec. shock (medium anxiety) and 300 sec. shock (high anxiety) on the other hand were to suppress subsequent food intake by 30 and 9 per cent. respectively. These suppressions were less of a deviation from normal than was the increase displayed by the low anxiety group.

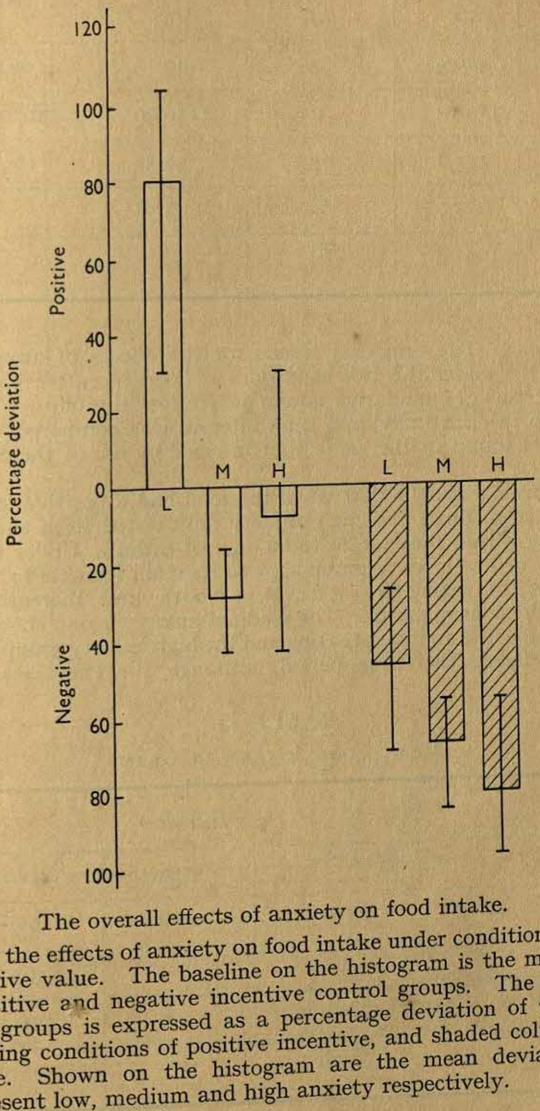
When the food had a negative incentive value (i.e. was adulterated with quinine) the effect of the three levels of anxiety were quite different. It can be seen from Figure 1 that they produced a suppression of subsequent food intake well below the baseline. This decrease in eating became greater as the level of anxiety increased (49, 70 and 83 per cent. respectively with low, medium and high anxiety).

These results strongly suggested that anxiety interacts with the incentive value of the stimulus situation to alter food intake when hunger drive is held constant. In order to ascertain whether or not these effects were statistically significant an analysis of variance was performed.

Shown in Table I is a summary of an analysis of variance for the mean food intake (Table II) in 23-hr. food deprived animals under conditions of varying incentive and following the induction of differing levels of anxiety. The main factors were positive and negative incentive and zero, low, medium and high anxiety.

It can be seen from Table I that the main factors of incentive and anxiety were both highly significant. This means, firstly, that the incentive value of the food, not surprisingly, affected the amount consumed, i.e. less food was eaten if the food had a negative incentive value; and, secondly, that the presence of anxiety in the animals also affected the amount that they ate. To understand the importance of the latter result, it is necessary to take into account the fact that the interaction between anxiety and incentive was also highly significant. This interaction indicated that the response curves for the effects of anxiety and incentive were not parallel, i.e. that the relationship between the amount of food eaten and the anxiety conditions varied with the incentive value of the food.

FIGURE 1



The overall effects of anxiety on food intake.

Figure 1 shows the effects of anxiety on food intake under conditions of either positive or negative incentive value. The baseline on the histogram is the mean food intake in grams for the positive and negative incentive control groups. The intake for each of the experimental groups is expressed as a percentage deviation of this baseline, clear columns representing conditions of positive incentive, and shaded columns conditions of negative incentive. Shown on the histogram are the mean deviations and ranges. L, M, and H represent low, medium and high anxiety respectively.

The analysis of variance thus demonstrated that the effects of anxiety suggested in Figure 1 were statistically significant. The presence of anxiety in the rats had a significant effect on their food consumption with hunger drive held constant, but the nature and extent of this effect depended on the level of anxiety and the incentive value of the food. Where momentary incentive value was positive, the effect of anxiety was to increase eating at the low level, and to suppress it at the intermediate and high levels. Where the momentary incentive value was negative, the effect of the three levels of anxiety was to produce an increasing suppression of eating.

In the analysis of results so far there has been no mention of the time course effects of the induced anxiety. In order to consider these effects an analysis of trends was made.

TABLE I

SUMMARY OF ANALYSIS OF VARIANCE OF FOOD INTAKE

Source	*S.S.*	*d.f.*	*M.S.*	*F*	*P*
Anxiety ..	420·4	3	140·13	39·1	0·001
Incentive ..	1136·3	1	1136·30	317·4	0·001
A × I	174·1	3	58·30	16·3	0·001
Error	222·7	62	3·58		
Total	1953·5	69			

Analysis of trends. The temporal trends for the effects of anxiety on eating are presented in Figure 2 under the two conditions of food incentive value. The amount eaten is shown in terms of cumulative intake curves for all groups. The intake of each experimental group in each individual time interval was compared with the intake of the relevant control group in the same time interval by use of the Student's t test for unrelated means.

It can be seen from Figure 2 that when the food had a positive incentive value, the suppressive effects of medium and high anxiety only lasted for a short period of time, tending to dissipate within the first 10 to 20 min. of eating. Under these conditions of positive incentive, the low anxiety group showed its main increase in food intake during the second 10 min. of eating ($t = 5{\cdot}24$, $p = 0{\cdot}001$), and thereafter did not differ significantly from the control group. The medium anxiety group showed a 100 per cent. suppression during the first 10 min. of eating and the high anxiety group also manifested its main suppression during this time period, although the effect was not statistically significant.

TABLE II

MEAN FOOD INTAKE IN GRAMS

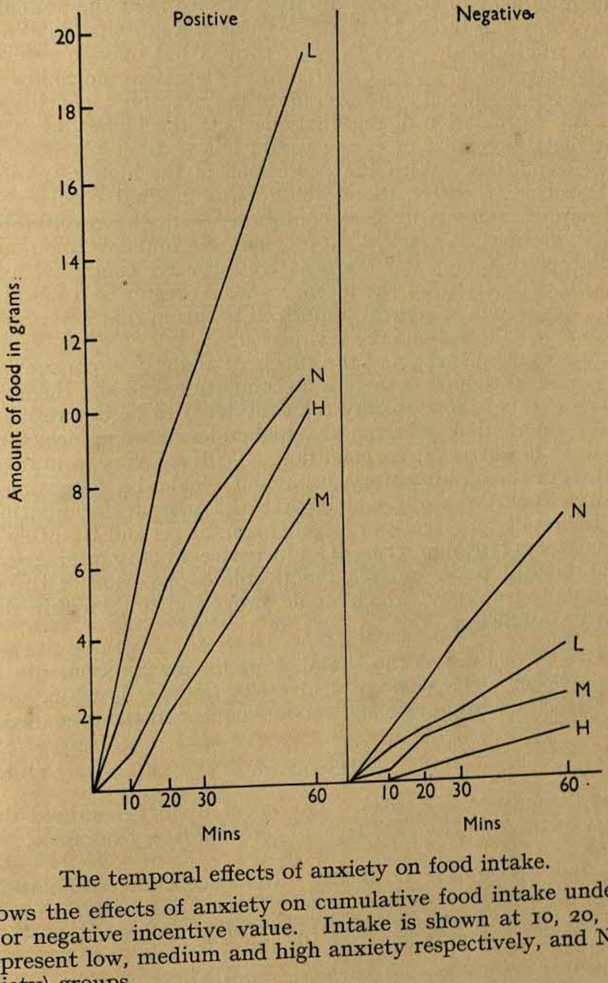
		Incentive	
		Positive	*Negative*
Anxiety	Zero ..	10·8	6·9
	Low ..	19·4	3·5
	Medium ..	7·6	2·1
	High ..	10·0	1·2

When the food had a negative incentive value the effects of anxiety took place over a longer time period, being spread fairly evenly over the most of the 1 hr. intake. The main suppressive effect shown by the low anxiety group under these conditions occurred during the second half hour of eating ($t = 2{\cdot}66$, $p = 0{\cdot}02$). The intake of the medium anxiety group differed significantly from the intake of the control group over almost the

whole hour (first 10 min.—$t = 5{\cdot}51$, $p = 0{\cdot}001$; third 10 min.—$t = 3{\cdot}31$, $p = 0{\cdot}01$; second half hour—$t = 6{\cdot}67$, $p = 0{\cdot}001$), whereas the high anxiety group showed a constant suppression over each individual time period (first 10 min.—100 per cent suppression; second 10 min.—$t = 3{\cdot}07$, $p = 0{\cdot}01$; third 10 min.—$t = 3{\cdot}75$, $p = 0{\cdot}002$; and second half hour—$t = 6{\cdot}67$, $p = 0{\cdot}001$).

From the above results the temporal trends of the effects of anxiety on eating may be summarized as follows: When the food had a positive incentive value the effects of anxiety on intake were generally short term, tending to disspiate after the first 10 or 20 min.; when the food had a negative incentive value, the effects were long term, tending to be fairly consistent over the whole hour of intake.

FIGURE 2



The temporal effects of anxiety on food intake.

Figure 2 shows the effects of anxiety on cumulative food intake under conditions of either positive or negative incentive value. Intake is shown at 10, 20, 30 and 60 min. L, M, and H represent low, medium and high anxiety respectively, and N represents the control (no anxiety) groups.

DISCUSSION

The results have demonstrated the influence on food intake of the state of anxiety existing in a rat at the offset of shock to be determined by the incentive value of the stimulus conditions. The lowest level of anxiety, under positive incentive conditions, produced a large increase in eating, whereas under negative incentive conditions a large suppression was evident. Thus the effect of low anxiety on food intake would appear to be due to an interaction of anxiety and momentary incentive value rather than an interaction with relevant drive conditions. Had the anxiety interacted with the hunger drive, then, whether the food were adulterated with quinine or not, intake would have risen.

The nature of the incentive-anxiety interaction was not defined by the present results, but could be tentatively considered as a threshold mechanism. The effects of a low level of anxiety would be to lower the threshold to the momentary incentive value of the stimulus, such that a positive incentive would lead to an increase in responding, and a negative incentive to a decrease in responding.

The role of the incentive-anxiety interaction was less clear for the medium and high levels of anxiety, where a suppression in food intake was found for both positive and negative incentive value. This suppression, however, was significantly greater for the negative incentive conditions. A possible interpretation of these results would be to assume that the intermediate and high duration of shock left the animals in such a state that hunger drive was effectively blocked. As this state dissipated as a function of time the hunger drive would again begin to exert control but only in combination with the incentive-anxiety interaction. From this point of view it would follow that there would be a greater suppression of food consumption in the negative incentive groups.

Inherent in this formulation of the dual effects of the higher levels of anxiety is the prediction that the suppression of eating would follow different time courses according to the incentive conditions. With the dissipation of the highly anxious state and the presence of a positive incentive there would be a gradual recovery from the initial suppression of eating. This is in close conformity with the results obtained from the positive incentive groups. A marked suppression was found at the start of the session. This faded rapidly with time. For the negative incentive condition, as the initial state dissipated the control exerted by the incentive would result in a maintained depression in food intake. The results in general conformed to this in that a maintained suppression of eating was observed throughout the test period.

It is clear from the explanation of the effects of anxiety on consummatory behaviour observed in the present study that there is no commitment to any theory of drive-anxiety interaction. The effects of low anxiety are considered to be determined in terms of an incentive-anxiety interaction, whereas the higher levels of anxiety are considered to induce a state which blocks any drive condition. With recovery from these drive blocking effects any subsequent consummatory behaviour would be determined again by the relevant incentive conditions. It is felt that the present formulation is capable of accounting for the results of Siegel and Siegel (1949), Siegel and Brantley (1951) and those of Amsel and Maltzman (1950) in terms of an incentive-anxiety interaction. It is thought further that the present paper offers a practicable explanation of the effects of higher levels of shock-induced anxiety, which would prove difficult for any theory predicated on a drive-anxiety interaction.

This study was carried out at the M.R.C. Unit for the Experimental Investigation of Behaviour, Department of Psychology, University College, London. I am indebted to Dr. Ian Steele Russell for his advice and encouragement in the preparation of this paper.

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A NOTE ON THE APPLICATION OF INFORMATION THEORY TO STUDIES OF TRACKING BEHAVIOUR

BY

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Human tracking behaviour is extremely complex. Many attempts have been made to obtain a theoretical understanding of it by using servo-mechanical theory. These attempts range from such early studies as those of Tustin (1944, 1947) to the elaborate investigations of McRuer and Krendel (1959). However, though not all workers would agree with Birmingham and Taylor (1954) that all such attempts are foredoomed to failure, no current formulation commands general acceptance; and few would reject the conclusion reached by Adams in his survey (1961) that "we are a long way from the beginning of any kind of theory of tracking."

In this situation a number of psychologists have turned to information theory as a hopeful technique for advancing tracking studies. Fitts (1954) examined a very simple acquisition task from an information standpoint. This will not be considered here as it has already been thoroughly discussed by Annett, Golby and Kay (1958), who emphasized the difficulty of finding a generally valid information measure, even in the simplest situations. However, Crossman (1960) reported an altogether more sophisticated information theory approach to tracking behaviour. This has not yet been discussed in the literature; but it contains a number of theoretical and procedural points that seem to require comment.

Crossman stated (p. 2) that he considered that three conditions must be fulfilled if information theory is to be applied validly to a tracking experiment. These are:

(i) "The course and the subject's performance must be statistically 'stationary' during a trial. . . ."

(ii) "The successive samples of the course must be, or appear to the subject to be, uncorrelated, so that the calculated input entropy shall fairly represent what he experiences."

(iii) "The Bandwidths of the course and track must be known and similar, so that the proper sampling interval for both can be calculated. . . ."

It appears to the writer that the second of these criteria may give rise to some confusion, as it is not invariably necessary, is in any case difficult to achieve, and may not have been achieved in Crossman's experiments. Further the third condition does not seem to be necessary either. These points require some elucidation.

That the successive samples should be uncorrelated is desirable, for it makes life easy, arithmetically speaking. It is, however, only *necessary* if (using the usual notation: p_i is the probability of the ith sample) the simplest entropy expression:

$$H = -\Sigma_i\, p_i \log p_i \qquad (1)$$

is employed, for this is only valid for uncorrelated samples. The more usual case, however, is for immediate successors to correlate fairly highly, and for the correlation to decay rapidly with separation.

In this case, a very long sequence can be divided into blocks of N samples each, N being large enough for the correlation between end members of each block to be negligible. Equation (1) can then be used to calculate the entropy per block. Calling the probability of the ith block p(Bi) the entropy per block will be:

$$H = -\sum_{i} p(Bi) \log p(Bi) \text{ bits per block.}$$

Consequently the entropy per sample will be given by

$$H = -\frac{1}{N}\sum_{i} p(Bi) \log p(Bi) \qquad (2)$$

(see, e.g. Pierce, 1962). The use of this expression would admittedly be tedious and difficult; but it is surely possible, obviating the general need for condition (ii) to be met.

As Crossman remarked, Poulton (1957) has demonstrated the remarkable ability of subjects to predict "what is coming next" in tracking situations. The existence of this ability, called "perceptual anticipation" by Poulton, clearly makes the achievement of condition (ii) very difficult.

The course used by Crossman was described by

$$i(x) = 1{\cdot}25 \sin x + 1{\cdot}10 \sin\frac{8x}{3} + 1{\cdot}27 \sin\frac{17x}{12}$$

where x is distance along the paper in units of 3·8 cm. Its autocorrelation function is:

$$A = 0{\cdot}356 \cos l + 0{\cdot}276 \cos\frac{8l}{3} + 0{\cdot}368 \cos\frac{17l}{12}$$

where l is lag along the paper in units of 3·8 cm. Crossman plots this expression as his second figure. (The plot is not very precise. The ordinate scale is different for positive and negative values; the fourth quarter of the abcissa is shrunk by about 30 per cent.; the value of A for a lag of 20 cm. is some way out, whether or no this shrinkage is allowed for. However, it is accurate for lags < 10 cm., which is all that matters here.) He declares (p. 4) that the maximum value of A for the first 25 cm. of course is $+0{\cdot}64$, and that at the sample intervals he chose (from 2·5 to 4·0 cm.) the correlation is about $-0{\cdot}4$. In fact, however, inspection of his curve shows that the maximum correlation is at zero lag, where it is unity; and that, although it falls away rapidly, its mean value in the range of lags from 2·5 to 4·0 cm. is not $-0{\cdot}4$ but about $+0{\cdot}2$. Also, it is difficult to agree with him that "it was reasonable to regard the course as unpredictable over any range greater than 2 cm.," for at that range the correlation is still about $+0{\cdot}6$. Thus it seems at least open to question whether condition (ii) was in fact met in the reported experiment.

It is true that, in order to calculate the correct sampling interval, the bandwidths of course and track must be known, at least approximately. However, there is no call for them to be similar, so long as the sampling interval is chosen for whichever had the higher bandwidth. In that case, a lot of the data obtained from the path of lower bandwidth would be redundant, but that does not invalidate the procedure. (Incidentally, the wavelength of the highest frequency in Crossman's course, $3{\cdot}8 \times \frac{3}{4}\pi$, is not 8·8 cm., but 8·95 cm.) It should, as a rule, be possible to ensure that the course's (known) bandwidth is the higher. Crossman's procedure, which was to smooth out higher track frequencies by eye (p. 7) was possibly acceptable for his particular experiment; but it is not a universally desirable one.

Another procedural question which may be important is that of the instructions given to the subjects. Crossman told them (p. 3) "to do their best to keep the pen point on the course, but that if unable to follow all its variations they were to attempt to reproduce it even if late." The second half of this instruction is most unusual. To reproduce a course, even if late, is not the same as to try to hang on to it, which is the usual tracking task. This alteration of the normal tracking task was, of course, necessary because of Crossman's decision to sample both course and track at corresponding turning points (pp. 4–5); and subjects could very easily miss out some course inflections if engaging in pure pursuit. However, this instruction makes it somewhat doubtful whether a typical, or representative, tracking experiment was carried out.

Crossman's study is a bold and ingenious one, but the factors discussed here indicate that his method and conclusions should be treated with some reserve, pending further and more extensive investigations.

The author wishes to thank Mr. P. Freeman, who has helped to clarify his ideas in discussion.

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A REPLY TO DR. MAX HAMMERTON

BY

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Dr. Hammerton has been kind enough to refer to the research reported in my paper (Crossman, 1960) as an "altogether more sophisticated information-theory approach to tracking behaviour" than that of Fitts (1954) and Annett, Golby and Kay (1958). Let me say at once that I make no such claim; the theory and results reported in my paper represent no more than the bare minimum needed to make a start on the informational analysis of tracking and I am aware that there is a whole series of loose ends and gaps in the argument, perhaps even enough material for a substantial thesis on "Errors and Omissions in Crossman's Information Theory of Tracking." All I hoped to do was provide an analytic framework and some pilot results on which subsequent workers could build and this, I think, the paper did.

Before proceeding to answer Dr. Hammerton's specific theoretical criticisms, I would like to set the record straight on the general evaluation made in his first paragraph. I for one entirely reject the conclusions drawn by Adams from his 1961 literature survey (Adams, 1961) and quoted with approval by Hammerton. Even at that time there was already a very considerable literature on the servo-mechanical theory of tracking, with results from which it had proved possible correctly to predict the stability and follow-up accuracy of manually controlled equipment and vehicles such as aircraft. Unless Adams (and presumably Hammerton) use the word "theory" in a quite unorthodox sense, it seems to me that their conclusion can only have been reached by failing to understand the significance of such contributions as those of Tustin (1947), Elkind (1953) and McRuer and Krendel (1959); indeed in my view the "servo-mechanical theory" of human tracking is actually very well advanced and powerful and at this stage no amount of psychological criticism can be expected to dislodge it. This is not to say that the theory is complete; as yet it applies only to a limited range of situations and it gives no account of the causal mechanisms underlying performance. But even in its incomplete form it provides the only systematic quantitative model of the human operator's tracking behaviour that we have, and we cannot afford to ignore it.

Against this background the informational approach that I have proposed should be considered as an alternative model describing the same performance in different parameters and not as a rival theory. It is evidently nonsense to assert that a human tracker actually *is* either a transfer-function or an information-channel, but both are convenient pegs on which to hang mathematical descriptions of the pattern into which his observed behaviour falls. Each provides approximations which are better for some purposes than others, and both will ultimately be "explained" and perhaps superseded by a unitary model incorporating a more detailed analysis of the receptor, central and effector mechanisms physically responsible for the tracking performance.

Referring now to specifics, Dr. Hammerton has quite correctly drawn attention to a lack of precision in the statements of my second and third conditions for validly applying information-theory to tracking experiments. I was fully aware at the time of writing that my verbal statements could only with difficulty be expressed in exact mathematical form, and I was also aware that the conditions of the experiment left much to be desired in all three directions. But my judgement was that the magnitude of errors caused by failure to provide exactly the right conditions was small compared to the inter-individual differences shown for instance in my Figure 7; and I do not think Hammerton has proved otherwise.

My condition (ii) requires that the calculated input entropy "shall fairly represent what the subject experiences." More explicitly this means that at any point in time the (subjective) distribution of expectancy of where the course will go in the immediate future shall not differ significantly from the (objective) probability distribution of where it can possibly go, derived from the actual distribution of sample amplitudes. A number of studies, for instance those of Poulton (1952), have shown that while subjects can predict the behaviour of a course consisting of a pure sinusoid over quite long time periods, they cannot predict that of one consisting of more than two sinusoids with non-harmonically related frequencies; so that "perceptual anticipation" should not occur in my case. I used a 3-component course, and while this is fewer than the ideal minimum of 5 or more recommended by some other workers, its subjective predictability was certainly small.

This claim can be supported theoretically by recalling that the information transfer r (x, y) between two continuous Gaussian random variables x and y is

$$R(x,y) = -\tfrac{1}{2}\log_2(1 - r_{xy}^2) \text{ bits.}$$

where r is the product-moment coefficient of correlation between x and y (e.g. Pinsker, 1964). Applied to the amplitudes of successive samples in my course, which are approximately Gaussian, a simple calculation from this formula shows that even a correlation of $r = 0{\cdot}8$ (the value of auto-correlation which occurs in my course at a lag of about 1·4 cm only provides the subject with an average of 0·73 bits of predictive power. At the mean spacing of my samples the available predictive information is certainly less than 0·25 bits, about enough to tell the subject which side of the centre-line the next sample will be on once in four samples. While this might possibly be detected experimentally by comparison with a more random course it is a small effect and I maintain that the experment actually did meet condition (ii) rather well.

It is true that the use of Hammerton's expression (2) could in principle eliminate this quite small error in estimating input entropy and I did consider it, but with the few data points available the greater statistical sampling error entailed in its use turned out to override the potential gain in accuracy by a large margin. However I would be the last to deny the need for better control over predictability and better estimates of entropy in future experiments.

Hammerton is theoretically quite correct in stating that my condition (iii) is not a necessary one as "there is no call for (the bandwidths) to be similar, so long as the sampling interval is chosen for whichever had the higher bandwidth." However the method of analysis implied by this choice of sampling interval turns out to be impracticable if one desires to discount the effects of variable transmission lag in the operator. It is in any case unnecessary since frequencies in the track exceeding the cutoff frequency of the course can only be due to noise added in transmission, and it is therefore permissible to filter them out without loss of information-capacity (MacDonald and Schultheiss, 1964). My procedure for doing this by eye is quite evidently "not a universally desirable one," but in the circumstances any other method would have been quite pointlessly cumbersome. In any case this question did not arise at the higher course speeds at which course and track had essentially the same bandwidth.

Dr. Hammerton's last point, concerning the instructions I gave the subjects to " . . . attempt to reproduce it even if late," is important. My experiment was in this respect not an ordinary tracking study. However my Figure 6 shows that in the preview condition subjects did not make significant use of the latitude thus granted them; and as my discussion pointed out (p. 15) the lag was much less than it could usefully have been even in the non-preview condition. Thus it seems that the instructions did not make much practical difference to the subject's performance, and they were actually cast in this form merely to discourage corner-cutting; which when it occurs is, of course, evidence of a much more sophisticated performance mechanism than either an informational or a servo-mechanical analysis can cope with!

May I say finally that there is room for much more research on the various topics raised in my paper and Dr. Hammerton's note. I hope soon to attempt a synthesis between the informational and servo-mechanical approaches to tracking, but whether or not it proves feasible, we need more data taken under better-controlled conditions, and more powerful methods of data-processing. Our best hope here seems to lie in the on-line use of digital computers for display generation and for processing operators output. So Dr. Hammerton can expect to get his teeth into some really rigorous experimental results in the not too distant future.

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BOOK REVIEWS

Thinking: From Association to Gestalt. By Jean and George Mandler. London and New York: Wiley. Perspectives in Psychology Series. 1964. Pp. x + 300. 38s. Cloth Edition. 23s. Paper Edition.

The plan of this book is excellent. It contains extracts from Aristotle, Hobbes, Locke, Hume and so on through the Würzburgers up to Wertheimer and Duncker, giving primary sources instead of the all too prevalent secondary ones. The inimitable *Rand* (The Classical Psychologists: Houghton, 1912) seems to be no longer available. It did start with Anaxagoras but it also ended with Wundt. The Mandlers' book is of course on a much smaller scale and much handier, and confines itself to the activity of Thinking.

One does wonder about some of the omissions and likewise some of the inclusions, especially in a work that has "concentrated on the traditional historical line" (p. VII). Is it perhaps censorious to ask: if Aristotle, why not Plato, whose pupil Aristotle was, and who seems to have given him the decisive hint for his theory of thinking? But it is definitely not true that "A thin line of scholarship connects these pages [concerning Aristotle] with . . . the British Associationists" (p. 13). The connection was the all powerful Church, which adopted Aristotle as the source of scientific knowledge. In an account of Associationism, running from Aristotle to Bain, one is entitled to expect an extract from Thomas Aquinas (1225–1274), who continually quoted both Plato and Aristotle: After all, *en route* to Bain, Leibnitz did remark in 1695, "When I had freed myself from the yoke of Aristotle"! The Church, with its broad sweep of powerful and acute scholars can hardly be called a thin line.

Roughly stated, the thesis of the book is that Aristotle laid down certain "laws of thought . . . essentially unimproved upon for two millennia . . . and still in use in recognizable form today" (p. 13), although there has been a progressive rebellion against them culminating in Gestalt theory. But as a matter of fact Aristotle laid down no such laws of thought. What he said was that thinking is akin to sensing, the *imaged* as object of thought being parallel to what we should now probably call the stimulus as object of sense. Just as we perceive the world without, so we *think* the world within, which consists of images. This is what he means when he says we cannot think without images, and when he speaks of the mind "in its world of images" (p. 9). δοκεῖ . . . καὶ τὸ νοεῖν καὶ τὸ φρονεῖν ὥσπερ αἰσθάνεσθαί τι εἶναι (thinking both speculative and practical are thought to be a kind of perceiving) though he says later that they are not the same thing as perceiving (427a). That this is fully accepted by Aristotle is shown by his statement (431a) τὰ φαντάσματα οἷον αἰσθήματα ὑπάρχει (things imaged are fundamentally like things perceived), [that is, when we are thinking]. Thinking consists of thoughts, that is, of images that have been subject to the process of thought. (Should we say today *processed by thinking*?). In an entirely different and apparently later monograph Aristotle discusses recollection and memory (449b), the former being the process by which we recover by means of our memories something we want to bring to consciousness (451b). It is here that the "laws of *Association*" are said to be found, and the main quotation from Aristotle in this book is headed *Recollection.* Hints for recollecting something 'lost in memory,' seem to have been elevated into "Laws of Thought." The statement that the mind thinks images may seem at first strange, until it is remembered that Locke stated in 1690 that *Idea is the object of Thinking* (Essay, book 2, ch. 1).

One is particularly glad to have available to students Titchener's statement on imagery (p. 167 ff.), on thought and on meaning. One welcomes also, translated for the first time, in the writer's knowledge, an important statement by Koffka on "concepts." It was of course proper to include a piece by Wertheimer, though whether his interesting defence of the occasional practical productiveness of the first figure of the syllogism ("Barbara") was the best selection from this powerful thinker may be doubted. With this—should one call it *jeu d'esprit*?—should have been paired the unanswerable attack by Husserl in the *Logische Untersuchungen.* One would like also to have seen included something of the wonderful Spinoza.

This all may seem a stepmotherly treatment of a book so sorely needed. But, regrettably, and in spite of the kind words accorded to the present writer's own work on Thinking, the reviewer does not feel that justice has been done to the excellent but supremely difficult task of illustrating from the primary sources the development of the Theory of Thinking.

G. HUMPHREY.

Risk Taking—A Study in Cognition and Personality. By Nathan Kogan and Michael A. Wallach. New York and London: Holt, Rinehart and Winston. 1964. Pp. 278. $8·0.

Anyone who reads this book would do well to start with the last section of the last chapter. Here he will find a lively and well-written argument for closely scrutinizing those who are entitled to press the buttons of nuclear bombs. Intuitively, the particular personality traits that the authors think are dangerous—anxiety and defensiveness—are the right ones to worry about; how the investigation in the rest of the book supports such intuitive judgements is not clear.

The authors argue that aspects of personality—in particular cognitive and emotional ones—should not be studied in isolation, and with this we must agree; their method is similar to that of a factorial experiment, and their results show that, in general, interactions are so important that main effects must be regarded as meaningless. A group of about 200 undergraduates were sorted into subgroups by sex, high and low anxiety, high and low defensiveness and for each subgroup correlations between scores on (mainly) paper-and-pencil tests and on responses in betting situations were found; the pattern of these correlations varied considerably from one subgroup to another, and this led the authors to conclude that subjects with high anxiety and defensiveness scores were likely to make irrational decisions.

The idea of a preliminary classification of subjects (by so-called moderator variables) has been used before, e.g. by Broadbent in investigating the effects of noise, and has not been found altogether satisfactory. It would seem possible to do a factorial experiment, combined with an analysis of covariance, even in this field, and results obtained in such a way could probably be interpreted with more confidence. The main question to be asked about the results reported is, however, whether we can extrapolate from immature students to presumably mature generals; from laboratory gambling games to real situations involving enormous risks; from group tests to clinical appraisement of personality; from American experience to that of other countries. The questions in the choice dilemmas procedure of Appendix E, for instance, give a picture of a curiously flat, materialistic world; they might well interest an anthropologist.

The preface and Chapters 1 to 7 are so written as to be almost unreadable; the lucidity of Chapter 8 shows that they are unnecessarily so and one wishes that the authors had taken more trouble to communicate with their readers. V. R. CANE.

Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology. Edited by T. W. Mann. London: University of Chicago Press. 1964. Pp. xi + 190. 37s. 6d.

This is the report of a symposium at Rice University, and was chosen by the Division on Philosophical Psychology "to mark its inception as a new division of the American Psychological Association." The symposium opens with Koch objecting to Behaviourism in his best polemical manner on the grounds that its methodological support is evaporating, and that its pragmatic consequences have been restrictive and unfortunate. MacLeod follows by presenting the challenge of phenomenology to experimental psychology. "I am merely insisting," he writes "that what, in the old, prescientific days, we used to call 'consciousness' still can and should be studied." Skinner then reviews "Behaviorism at Fifty." He defends psychology as a science of the behaviour of organisms, and points to the way in which, he believes, private events (of, for example, "conscious content" and expectancies) can be dealt with by a science of behaviour. Next, Carl Rogers argues that psychology should become "a more inclusive science" by allowing not only for "objective knowing," but also for "subjective knowing" (e.g. "Yes, I do love her"), and for interpersonal knowing, or phenomenological knowledge (e.g. "I know that you really despise yourself"). The symposium concludes with two papers from philosophers—Malcolm and Scriven. The gist of Malcolm's paper consists of an application of the argument of Wittgenstein in the *Philosophical Investigations.* "Behaviorism is right in insisting that there must be some sort of conceptual tie between the language of mental phenomena and outward circumstances and behavior," otherwise a sentence such as "I am now excited" could not logically be understood by other people or by the person himself. But Behaviourism is wrong in supposing that this first person sentence has the same "content" as the third person sentence "He is excited now," because sentences of the former class (unlike those of the latter) are *not* "based on observation" at all, whether of outward events or "of physical events inside the speaker's skin." Scriven maintains that as a science, psychology is subject to certain limits—its territory is restricted by common sense, it is restricted and constantly being annexed by other sciences, and the controlling variables lie at least partly outside the range of the psychologist. Psychology will never, therefore, be in a very different state from what it is in at present; and Scriven advocates what he calls "a local government point of view for psychology." Each paper is followed by a short paraphrase of the subsequent discussion.

This volume is an interesting and in several ways a thought provoking production; and the meeting itself must have been a valuable occasion to the participants. But it does not read very happily as a book. Thus, it raises a large number of different issues of considerable complexity, none of which is adequately examined or discussed. Each of the four psychologists is concerned, in part, to support a certain programme of research and development in psychology; and the way in which each talks about psychology forms part of his concern to back his own, to him, progressive research strategy. But in doing this the psychologists do not seem to me to reach, in general, the level of caution and penetration that the difficulties of the subject really demand. What is more, some of the considerations they do raise seem to me to be remote from what psychologists are actually doing and thinking about at the present time. (Consider, for example, MacLeod's contention that old fashioned consciousness should be studied. What *does* he think psychologists are doing when they study auditory and visual perception, for instance, or pain reactivity, absolute judgements, aphasia, etc., etc.? "Consciousness" is not a field in its own right, waiting to be studied as soon as MacLeod can induce psychologists to turn to it and to stop investigating other subjects such as perception.) Some years ago Ryle argued that there are no "isms" in philosophy. I feel like protesting here that there should be no "isms" in psychology either, and that there are none for the ordinary working psychologist. Unfortunately, this book may encourage us to suppose that the banners of "isms" and "ologies" are indeed widely raised across the psychological landscape; and to suppose, further, that it is quite appropriate for us to continue to raise and waive them.

The papers of the two philosophers are quite different in character from the rest, and I shall restrict myself to making one point about each. Malcolm's case against Behaviourism seems to me to be a confusion. It is quite possible for us to agree that "I am excited now" is not "based on observation," and yet to argue that this sentence is not saying anything other than that a certain person, namely B.A.F., has reacted, is reacting, and will react in certain ways, namely, . . . And this is all that a Behaviourist would normally want. But whether a reactivity analysis of first, or third, person sentences is good enough to save Behaviourism depends on how we use the word "behaviour" in our statement of this "ism." Obviously, if we use it ordinarily (to refer only to items such as gross movements of the body), then Behaviourism cannot be saved by a reactivity analysis. If, however, we use the word in a technically familiar, but extended sense (to include the minute peripheral and central activity of the organism), then we can save Behaviourism in this way. But is this verbal matter very important? Why bother about saving or not saving an "ism"? Scriven's paper has the great merit of showing that he is thoroughly familiar with what psychologists are doing. His deflationary protest against large theories, and high expectations in psychology is a very salutary protest both in the U.S.A. and also perhaps in Great Britain—where the development of new techniques has tended in recent years to generate in psychologists a somewhat grandiose view of the status and prospects of their subject. I am not sure that Scriven presents his deflationary protest as clearly and cogently as it might be done. But this defect is one to which, no doubt, Scriven would have been able to attend if he had had more space.

Perhaps there is a general moral in all this for the organizers of symposia. Arrange for them to cover small rather than vast areas, plan them very very carefully, and give symposiasts and discussants the space in which to come to grips with the issues that they have been gathered together to discuss.

B. A. FARRELL.

Psychology and Social Problems. By Michael Argyle. London: Methuen. 1964. Pp. 232. 25s.

Dr. Argyle's book is an attempt to present a concise summary and discussion of recent research by social scientists concerned with some prominent social problems and with processes of social control. The text runs to almost exactly 200 pages, with a bibliography of another 23. These statistics are mentioned because they indicate the scale and scope of the book, and suggest that in some ways the author has set himself an impossible task. Within this compass he discusses 16 different topics. These cover most of the major social problems, although sometimes they may be considered from rather specialized angles, education, for example, being treated most fully under the heading of selection.

The problems of such a concise discussion inevitably centre on the choice of sources, the extent of simplification, the use of language and concepts which the reader might, or might not, be expected to understand and, in general, assumptions about who is going to read the book.

The compression involved means that not all sources can be used. On delinquency, about which this reviewer is best informed, it would seem that the omissions result from an emphasis on an experimental or quasi experimental approach which is thereby given a somewhat greater importance than many teachers of the subject would accept. In itself this does not matter very much, but insofar as Argyle claims in his Preface to speak on behalf of "social scientists" it is somewhat misleading.

A more serious weakness is that the conciseness sometimes leads to more categorical assertions than the evidence would appear to justify, e.g. ". . . delinquency and inadequate conscience-formation will result if discipline is feeble or inconsistent"; or, ". . . Catholics do not demand independent achievement at such an early age as do the other groups. This explains the slow progress of industrialization in Catholic countries like Ireland, Spain and Italy. . . ." Undoubtedly there is something in such assertions, but the confident lack of qualification is slightly disturbing. Similarly there is a tendency to take up a topic, mention a few points, and drop it, all within a short space—national character, for example is covered by one paragraph on p. 31 and less than a page elsewhere.

Linked with this is the major problem of language. The discussion makes fairly free use of technical terms and of sophisticated concepts, as in the opening chapter on Personality, and it is doubtful whether a layman would understand it. On the other hand it seems improbable that the reader capable of fully understanding the exposition would be told anything new in a summary of eight pages. This, no doubt, is the central dilemma facing any popularizer and Dr. Argyle does not seem to have solved it.

Possibly the most fruitful approach would be to regard the whole book as an extensively annotated bibliography. It would lead ultimately to many of the important sources and, in this way, might be suitable as a guide to specialist students wanting to broaden their approach. Apart from the difficulties of the conceptual level, the book is pleasantly readable. J. P. MARTIN.

The Abnormal Person and His World: An Introduction to Psychopathology. By Paul J. Stern. The University Series in Psychology (Editor: D. C. McClelland). Princeton and London: Van Nostrand. 1964. Pp. xi + 234. 40s.

The author of this book, who was educated in Brussels and Zurich as well as at Harvard, takes the fashionable "dynamic" view of mental illness but with a strong admixture of psychiatric existentialism. This, and the fact that it is well written, render it a good deal more interesting than most introductory texts of its kind. As might be expected, the author has little interest in bodily factors in mental illness or sympathy with methods of physical treatment. And he shows an almost total disregard for the methods of experimental psychology. None the less, this book is well worth reading and may be recommended to students, particularly at postgraduate level.

O. L. ZANGWILL.

Work and Motivation. By Victor H. Vroom. London and New York: Wiley. 1964. Pp. ix + 331. 53s.

This book deals with choice of job, job satisfaction, the relatively small effect of job satisfaction upon behaviour at work, and the relationship of performance to motivation. In discussing motivation the author uses the concepts of subjective value and subjective probability. By this means he makes theory explicit in a field of industrial psychology which has been indigestibly empirical. He emphasizes individual differences in motives, and notes an interesting interaction between motivation, ability and performance: increased motivation improves the performance of the able, but not of the less able. There is a useful concluding chapter on method and theory. The book is extremely well written and documented. It can be strongly recommended to students of applied psychology and industrial relations. E. C. POULTON.

The Physiology of Diurnal Rhythms. By Janet E. Harker. Cambridge Monographs in Experimental Biology, No. 13. London: Cambridge University Press. 1964. Pp. vii + 114. 20s.

This book displays all the advantages of being written by an expert. Dr. Harker has devoted a great part of her research career to the study of diurnal rhythms, and her book is an excellent critical review of all the major types of experimental investigation in this active field. She chooses explicitly to consider . . . "a rather limited range of processes and organisms in order to review in detail the properties of rhythms." Nevertheless, the range of both is impressively large and no reader will be able to skip through the tightly written account without missing a great deal.

The author deals briefly with terminology and at length with the environmental control of rhythms, free-running rhythms, phase shifts, and possible physiological control mechanisms. The last named chapter is the highlight of the book, perhaps because it is in this area that the author has made such notable experimental contributions of her own. There is also a short account of abnormality and disease in rhythmical systems, and a concise summing up of the important empirical and logical problems associated with any attempt to study rhythmic processes in living organisms. For example, the discussion about the control and investigation of all possible external triggering stimuli like magnetic fields and cosmic rays is particularly useful.

I have only two small reservations. The first is that no reference is made anywhere in the book to two closely related monographs by Cloudsley-Thompson and by Bünning. Someone seeking general surveys of this field might well find them useful. The second is that Hoagland's classic experiments on the temperature dependence of time perception in man are nowhere discussed or referred to, despite the fact that there is a discussion of the effects of temperature on rhythms. But these are minor points and do not lessen the statement on the dust cover that this book will appeal to experimental psychologists. It will.

ALAN COWEY.

Motivation: Theory and Research. By Charles N. Cofer and Mortimer H. Appley. London and New York: Wiley. 1964. Pp. 958. 94s.

When assessing the effects of independent variables on learning, psychologists often meet the problem of distinguishing changes in performance caused by interference with learning from those due to altered motivation. The last chapter of Cofer and Appley's book *Motivation: Theory and Research,* serves as a warning not to let such practical preoccupations solidify into assumptions about the independence of certain motivational phenomena from learning. The authors discuss, for instance, how sexual behaviour can be conditioned to location and point out that as yet there is no evidence that copulation alters androgen levels in the blood; they stress that there are adjustments to feeding schedules, and that giving an animal a small amount of food prior to maze running may increase its speed, invigorate it. An "invigoration occurs, not as an inevitable consequence of deprivation but as an effect of anticipation instigated by stimuli that have accompanied feeding." The *invigoration mechanism,* which can be brought into operation either by these anticipations, or by sensitization due to internal physiological changes, is thought of as functioning in part as generalized drive (*D*) was supposed to function, "thus preserving one of the most interesting aspects of drive theory—the separate mechanism governing learning and performance."

But, as the authors admit, their proposals for an "interpretive reconceptualization" are not very specific. It is not clear how these invigorations operate at a detailed level, or what laws might determine the extent to which they will form part of a generalized drive. This book therefore will be more useful as a description of research than as a theoretical contribution. The various ways of treating motivational phenomena are covered in the main bulk of the book: the ethological attitude, the physiological approach concerning internal states, homeostasis, motivation in learning theory, the views of psychoanalysts and some aspects of social motivation. A profuse variety of material is presented ranging from an outline of existentialism to the details of hoarding and migration.

As a review of the major research and thought in these areas the book will be of practical value, and is supported by a superbly complete bibliography of almost a hundred pages. Much thought has gone into the organization of the material. Unfortunately insufficient attention has been devoted to the fine grain of the text and the style makes the book longer than it need have been.

N. MROSOVSKY.

The Design of Electric Circuits in the Behavioral Sciences. By Tom N. Cornsweet. London and New York: Wiley. 1963. Pp. xi + 329. 68s.

This is a disappointing book. There is certainly a need for a good book to help the ordinary experimenter in psychology to find his way about the maze of modern technology, but this book does not fill the need. In content it is much too elementary and incomplete, and in presentation it is very poor. A. CARPENTER.

Stability and Change in Human Characteristics. By Benjamin S. Bloom. London and New York: Wiley. 1964. Pp. xiv + 237. 53s.

Professor Bloom achieves a Herculean task: an analysis of all the longitudinal studies relating the measurable qualities of children to those the same individuals display as adults. Specifically, he encompasses development in height, weight, strength; intelligence; academic ability; interests, attitudes and personality. He finds, broadly speaking, that all of these characteristics conform to a single pattern. Each shows a surprising degree of stability from relatively early stages in life. Thus some 50 per cent. of the variation in adolescents' height is already accounted for before they are three. Similarly, some 50 per cent. of the average adolescent's ability to do intelligence tests seems to be determined by the time he is 4. And roughly a half of the variation amongst adolescents in intellectual interest, dependency and aggression is predictable from the age of 5.

Although his enormous feat of compilation is based, as Professor Bloom readily admits, upon the relatively superficial qualities which we can measure with tests, the finding is one of profound significance. It provides strong (though qualified) statistical support for what psychoanalysts have believed all along: that the first 5 years of a child's life are crucial to his later development. This is a finding to cause satisfaction in psychotherapeutic circles, and despair amongst teachers—for if adult behaviour is so heavily determined and so early, there remains depressingly little for the schoolteacher to do. These, however, are not the morals which the author himself chooses to emphasize. Indeed, precisely the reverse. He advocates (and, surely, he is right to do so) not the meek acceptance of what inheritance and early environment choose to provide, but a determined attempt to measure, understand and eventually manipulate that early environment to our own advantage. In this respect, the book is thoroughly modern. It indicates the direction in which the psychology of human nature is now developing—away from the passive acceptance of the "environment" as a nebulous, inscrutable force; towards the detailed descriptive analysis and experimental control of the various kinds of environment which our society provides. As Professor Bloom remarks, there are not "good" environments and "bad" ones: rather ones which favour or prejudice the development of particular physical, intellectual and emotional characteristics. It is such specific relationships which his book helps to illuminate. L. HUDSON.

Hypnotic Induction of Anxiety: A Psychoendocrine Investigation. By E. E. Levitt, H. Persky and J. P. Brady. Springfield, Illinois: Thomas. 1964. Pp. xvi + 134. $6.50.

This book reports work in which a state of anxiety is induced in volunteer subjects under hypnosis. Questionnaire and projective test results given in anxiety and control conditions tend to support the idea that some sort of state of anxiety can be successfully induced by hypnotic means. On the other hand, levels of plasma hydrocortisone, blood corticotrophin and plasma adrenal weight-maintenance factor which the experimenters hoped would reflect a state of anxiety were not reliably changed by the experimental technique.

The experimenters go to the trouble of involving subjects in complicated and time consuming studies, and are sophisticated about the pitfalls of the use of volunteer subjects some of whom are and some not susceptible to hypnosis. What is then disappointing is that they do not then make more use of their subjects by taking other measures of, for instance, psychophysiological state, or perceptual or motor performance. None of us can be excited by the fact that in a state of induced anxiety, anxiety was reported. In all, five experiments are reported in some detail, and summarized and discussed in more detail. The dullness of the findings does not seem to have justified the trouble of writing a report of book length. P. H. VENABLES.

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RESPONSE LATENCIES IN NAMING OBJECTS

BY

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After some preliminary analysis of what is involved in naming objects, in which the possible role of classificatory systems in the memory store is discussed, it is shown experimentally that there are consistent differences between the times taken to respond to presented objects by uttering their names, variations between the performances of different individuals being outweighed by variations due to the different objects. Moreover, there is a high consistency between different individuals as to the ordering of objects in respect of their naming latencies. It is further shown that a high correlation exists between the time taken to name an object and the frequency with which its name occurs in the language as a whole, as estimated in the Thorndike–Lorge Word List. Some implications of these findings are discussed, especially with reference to possible mechanisms by which presented objects are visually identified, and the appropriate names retrieved from the "word-store."

INTRODUCTION

When we try to consider in any detail how appropriate words are chosen and produced in written or spoken discourse we are bound to contemplate, first the existence of some kind of store from which items are drawn, and secondly, some process by which the appropriate one is selected. In ordinary speech or writing a number of factors such as the existence of contextual, grammatical and syntactical constraints assist this choice. The ways in which these factors operate have been clarified by Shannon (1948), Taylor, W. L. (1953), Taylor, A. and Moray N. (1960) and other workers. It is now accepted, for instance, that much we say or write is determined by the statistical structure common to any discourse which is in practice meaningful, rather than by the particular proposition we seek to convey. To the extent that language is thus redundant, the act of choosing each individual item requires the processing of less information than it would in a symbolic system containing no interdependencies.

There is, however, one special case of word-production in which little recourse to context is possible and where the task is far more specific than it is in continuous discourse. If an individual is presented with an ordinary object and asked to give its name, he is faced with a situation in which there is no verbal context to provide partial determination of his choice. There are, of course, a number of objects which have more than one reasonably appropriate name and also a number which share their name with others. But in general a person asked to name an object faces a specific question to which there is only one correct answer. Presented with a chair, the responses "table" and "monkey" are equally inappropriate.

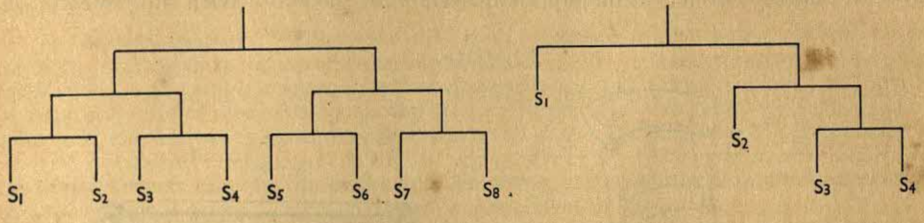
A rough sample count of the Concise Oxford Dictionary for words which could be correct answers to the question "What is this"? when some object is displayed gives a figure of about 5,000 items. To produce the correct answer to such a question an individual would have to discriminate between this number of possibilities. If he were initially quite ignorant as to which of them was to occur he would have to process about $\log_2$ 5,000, or just over 12, bits of information to identify every single item. Such complete uncertainty might be maintained in a laboratory experiment. But, in any everyday situation in which an individual is called on to name things, the relative frequencies of occurrence of the objects in past experience, and of use of the name-words will introduce some unbalanced frequencies and hence reduce the number of bits to be processed in the task. We may, nevertheless, note in relation to the figure of 12 bits mentioned above, Treisman's (1964) estimate for the mean information yield in supplying single missing words from a passage in a novel by Joseph Conrad. Her figure for this is about 2½ bits.

The special difficulty experienced by dysphasic patients in naming objects has long been known. In such individuals words may be freely available provided their production takes place within a verbal context. On the face of it, it is the specificity of the task which renders it difficult. The patient has no aids (such as those deliberately introduced into a graded series by Rochford and Williams (1962) in an experiment on dysphasics) and must produce the one, and only one, word from his repertoire which fits the particular object with which he is presented. The outcome is often circumlocution—descriptive rather than denotative in intention. In the course of such circumlocution the patient may freely use words which, as names in the naming situation, he is unable to evoke. As Head (1926) (Vol. I, p. 397) remarks, ". . . the name of an object can be embodied in a sentence, although it could not be formulated immediately as a single word." These easily observable facts suggested to some investigators in this field that, as Goldstein (1948) (p. 61) remarked, "The nature of naming is often misinterpreted because one does not differentiate between naming and simple association of a word with an object." It has often further been argued that to name an object involves some degree of abstraction. Goldstein (p. 61), for instance, remarks that "One may be surprised that naming demands an abstract attitude" and instances various pieces of evidence to show that the failure of patients in this task often points to the breakdown of capacity to maintain such an attitude. Goldstein's conception of the "abstract attitude" has not gained universal acceptance, but similar suggestions that the application of a name to an object in virtue of the latter's membership of a certain class involves some abstraction are wide-spread in the literature. Head, for instance, compares disabilities in object-naming with those manifested in naming colours. "We speak of a name as concrete, when it covers a small group of 'objects'," he writes (Vol. 1, p. 526), "whilst a more abstract designation is applicable to a wider range of events. Thus the word 'knife' is more concrete than the word 'red.' For the former can apply to a small group of things only, which have many characters in common, whilst the latter expresses a 'quality' to be discovered in articles of profoundly different use, shape and texture."

Such features in the naming task strongly suggest that it involves some form of progressive analysis of the presented characteristics of the object. Apart from any question of searching for the appropriate word, a thing must, as Woodworth (1938) remarks, and as Cattell (1886) first appreciated in pioneer experiments in this field, be identified before it can be named. One is reminded of the sequence of decisions involved in following through a botanical key to identify and name a flower, though in such a case the end-point of the procedure affords not only the identification but also the name. In general we may note that, given a classificatory system, whether or not there exists legitimate and effective short cuts in particular cases, a certain minimum number of decisions as to the presence or absence of a set of detectable characteristics is necessary to arrive at an identification and a name.

Any elaborate analysis of hypothetical classification schemes is beyond the scope of this paper, but some elementary consideration of a few simple examples will serve to sketch in the background of the experiments to be described. Consider first a fully dichotomous scheme as in Figure 1 (A).

FIGURE 1



(A) (B)

All the 2^n species discriminated as a result of n decisions lie at the same distance in terms of the number of decisions required to reach them, from the starting point. If the time taken to reach each decision were constant all the species would take the same time to identify. So far as the identification part of the process of naming is concerned, therefore, there would be no variance in the latencies.

No botanical or similar key, however, possesses this simple completely dichotomous structure, and it seems certain that no individual's memory store does either. An opposite extreme is shown in Figure 1 (B), where n decisions discriminate only $n + 1$ species. It is clear that time-variance will be introduced. In this connection we have to take into account the different probabilities of occurrence of the different species. The most economical key, for instance, will be that in which the commonest species occur at the beginning and the least common at the end, the most time-consuming that in which the opposite is the case. (It is worth remarking that standard botanical keys, based as they are on traditional morphological characteristics, by no means conform to a criterion of economy.)

For this reason, it is of interest to examine the correlation of latency in naming with some measure of the commonness of the objects named. No explicit information as to the relative frequencies with which different objects are encountered is available and we shall instead take into account the *frequencies of the words used in naming them*, as given in the Thorndike-Lorge (cited later as T–L) Word List (1944).

The 26 objects we used in the main experiment were chosen so as to cover a wide range of frequencies of occurrence of the words serving as their names. It has to be remembered that entries in the Thorndike-Lorge List represent *all* uses of a given word—not merely its use as a name. It should further be emphasised that the frequency with which a picture of a given object is encountered by no means necessarily tallies with the frequency with which the object itself is seen in everyday life. As will be seen, however, in spite of these factors, a close correlation subsists between naming-latency and T–L frequency.

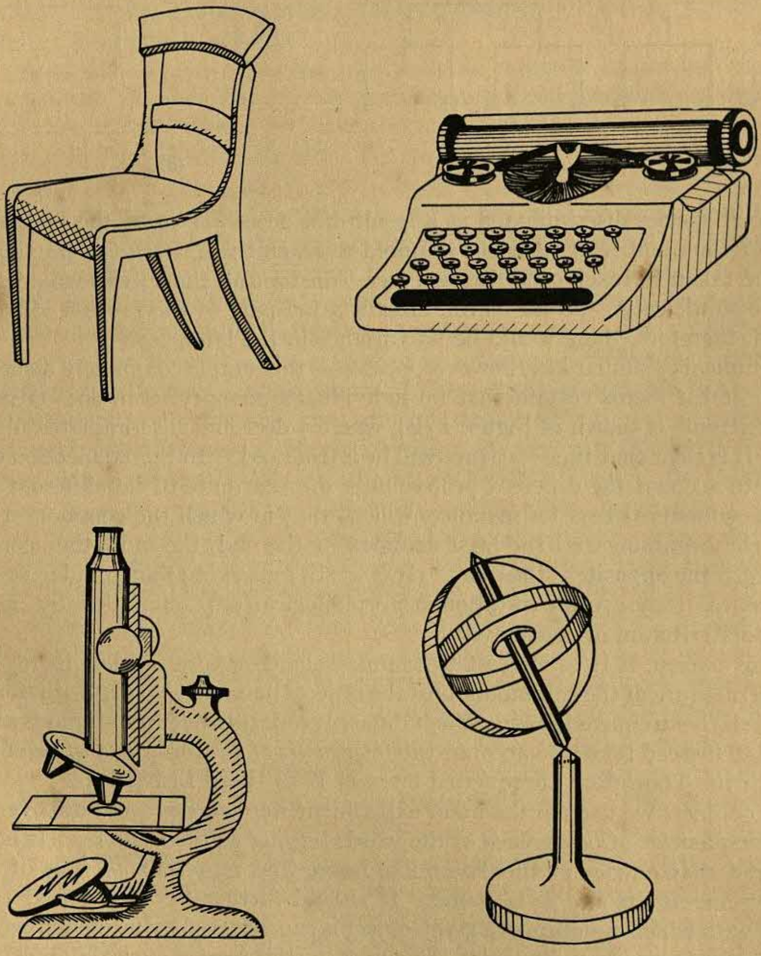
Experiments

An extensive preliminary experiment was carried out, using the methods described below for the main experiment, which involved the naming of some 80 objects by five subjects. The 36 objects used in the main experiment, 10 of which served as practice items, were chosen from these 80. Since the procedure was virtually the same, while the subjects and the actual pictures of the 36 objects were different in the preliminary and main experiments, the results of the former provided an important check on the latter and some of these preliminary data are shown for comparison. These experiments were briefly reported in Oldfield and Wingfield (1964). The two sets of results will be referred to as Series I and Series II.

(*a*) *Method.* After the pilot study had established the difficulties and requirements to be met in the main experiment, the following method was adopted:—

Objects were presented as simple black outline drawings by projection onto a translucent screen, 8 cm. × 12·5 cm. The subject viewed this from a distance of about 50 cm., the object outline thus subtending approximately 8° at the eye. With the projector off, the screen, surrounded by a black surface, was illuminated only by diffused light from a single "Angle-Poise" lamp not in view of the subject. The brightness of the screen in this condition was 0·1 millilamberts. The stimulus picture was presented by abruptly and silently raising the supply voltage to the projector lamp to its full mains value from one sufficient to maintain it at redheat but below threshold. The time taken for the lamp to reach full light intensity was measured and found to be 60 millisec. The delay between onset of full lamp voltage and operation of the pen recording was 20 millisec. We have no means of telling, of course, whether any part of the perceptual or naming process takes place before the full lamp voltage and picture brightness is attained, but even if none does the errors of latency measurement, introduced by the apparatus are constant and not

FIGURE 2



greater than 80 millisec. With the projector on, the screen brightness in the background of the outline picture was 5·0 millilamberts.

The subject responded vocally and the signal from a microphone held in his hand was amplified and fed to a high-speed relay which in turn applied an impulse to the pen of an ink-writing oscilloscope. Using this arrangement, the onset of the spoken verbal responses were recorded in a standard fashion irrespective of their different acoustic energy characteristics. The onset of the stimulus was recorded on a second channel of this instrument and with a paper speed of 10 cm./sec. latencies were measured to the nearest 10 millisec.

(*b*) *Stimulus pictures.* Thirty-six objects were chosen from a larger number by preliminary trials with the following considerations in mind: (1) ease of portrayal in outline, (2) possession of a single commonly acknowledged name, (3) lack of similarity to the other objects, and ease of identification in the absence of pictorial "context," (4) formation of a collection, the frequency of whose names, as given in the Thorndike and Lorge (1944) List, were evenly spread throughout the whole frequency range. In the preliminary experiment the drawings were done by one of the experimenters, in the main series they were executed by a professional artist. Four of the latter are shown in Figure 2.

Ten of these pictures were used as a preliminary practice run with each subject. The objects in this series were bed, boat, watch, telephone, pencil, piano, comb, skull, scissors, toothbrush. The objects in the remaining—experimental—series of 26 are detailed in Table I, with their corresponding T–L frequencies of occurrence per million words.

(*c*) *Subjects.* 12 normal adults, 11 male and one female, were used. Their mean age was 25·5 years, range 20–42. Four were university graduates, five undergraduates; two of the remainder had had non-university higher education. With two exceptions, all were connected with this Institute in various capacities. None was aware of the specific theoretical background to the experiment.

(*d*) *Order of presentation.* Each picture in both practice and experimental series was presented once only at intervals of about 5 sec., in a randomized order obtained from the usual random number tables. The order was different for each subject.

(*e*) *Instructions.* The following instructions were given orally to each subject:—

> "This is a test of your ability to name objects. You will see on the screen in front of you a series of pen and ink drawings of objects similar in style of drawing to this one. (Picture of a guitar shown.) In all cases the pictures will be of objects you should be able to recognize, and for which there is really only one name that best fits it. When the picture appears, you are to give the *correct* name for the object as *quickly* as you can. We shall now have some pictures for practice."

(*f*) *Effects of practice.* To gain an estimate of the extent to which practice might, by providing some acquaintance with the ensemble, alter response-latencies, the experiment was repeated twice on five of the subjects, the presentation order being changed in each case. Intervals between trials were as follows:—

	Intervals (*days*)	
Subject	*First to second*	*Second to third*
1	1	4
2	4	3
3	1	1
4	2	5
5	1	1

RESULTS

The principal results concern (1) Variation of naming-latency as between different objects; (2) Consistency as between experimental subjects regarding such variation; (3) Relation of latency to word-frequency.

Mean latencies for the 26 different objects are given in Table I, for Series II, together with the T–L word frequencies. Rank orders in respect of latency are also shown, while the mean latencies for Series I are given in brackets for comparison.

TABLE I

Frequency	*Rank*	*Stimulus picture*	*X latency*	*Rank*	*N*	*σ*
AA	2	Book	710 (620)	9·5	12	119·5
AA	2	Chair	550 (650)	1	12	65·4
AA	2	Shoe	590 (620)	2	12	156·1
A	5	Basket	640 (630)	4	12	111·7
A	5	Clock	690 (600)	7	12	90·6
A	5	Key	630 (650)	3	12	157·4
43	7	Glove	680 (760)	5	12	106·4
40	8	Drum	690 (660)	7	12	136·2
32	9	Tap	710 (690)	9·5	12	135·4
26	10	Anchor	760 (790)	13	12	172·6
22	11	Cigarette	820 (770)	15	12	119·2
20	12	Screw	840 (750)	17	11	156·9
12	13	Typewriter	820 (790)	15	11	186·1
11	14	Windmill	690 (1080)	7	12	113·1
9	15	Microscope	960 (1000)	18	11	251·6
8	16	Dice	720 (680)	11	12	114·3
7	17	Anvil	1020 (910)	21	12	156·1
4	18	Horseshoe	980 (720)	20	12	406·8
2	19	Octopus	750 (820)	12	12	134·6
1	20	Bagpipe	820 (930)	15	12	208·6
0·94	21	Tuningfork	1140 (1120)	23	11	261·6
0·88	22	Stethoscope	1360 (950)	25	10	877·4
0·83	23	Syringe	1080 (1210)	22	11	316·1
0·38	24	Metronome	970 (1120)	19	12	207·4
0·33	25·5	Gyroscope	1680 (1140)	26	10	710·1
0·33	25·5	Xylophone	1240 (1240)	24	11	401·2

Two-way analysis of variance for subjects v. objects, given in Table II, establishes that both these factors are significant, the greater amount of variance being contributed by the objects. Since the mean variances are tested against the remainder which incorporates any subject-object interaction which might be present, this result makes it clear that the different subjects are significantly agreed as to the ordering of objects in respect of their latency. It does not, however, reveal the extent of this agreement and to do this Kendall's coefficient of concordance W has been calculated. A few ranks are missing owing to failures to respond. We accordingly calculated two values for W—one giving the missing items the highest possible, and the other giving

TABLE II

Source	*Sum of squares*	*d.f.*	*M.S.*	*V.R.*	*Significance*
Subjects	4,745,851·00	11	431,441·00	6·251	$p < 0·001$
Objects	18,678,381·90	25	747,135·28	10·825	$p < 0·001$
Remainder	18,980,511·80	275	69,020·04	—	—
Total	42,404,744·70	311	136,349·66	—	—

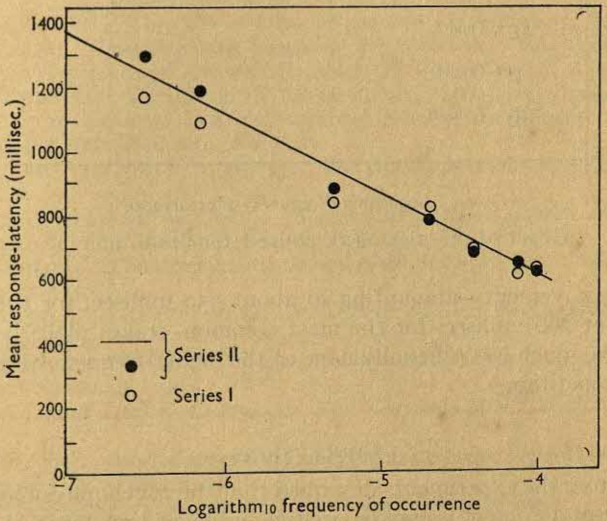
them the lowest possible, ranks. We also calculated W for those six subjects for whom a complete set of responses was available. Table III shows the values of W obtained, together with the corresponding values of z.

TABLE III

	n	W	z	*Significance*
Missing items ranked low	12	0·43	1·058	$p \leqslant 0{\cdot}001$
Missing items ranked high	12	0·56	1·325	$p \leqslant 0{\cdot}001$
No missing items	6	0·70	1·623	$p \leqslant 0{\cdot}001$

We may therefore conclude that naming response-latencies are strongly dependent upon the object presented, and we now turn to the relationship of latency to frequency of the name-word.

FIGURE 3

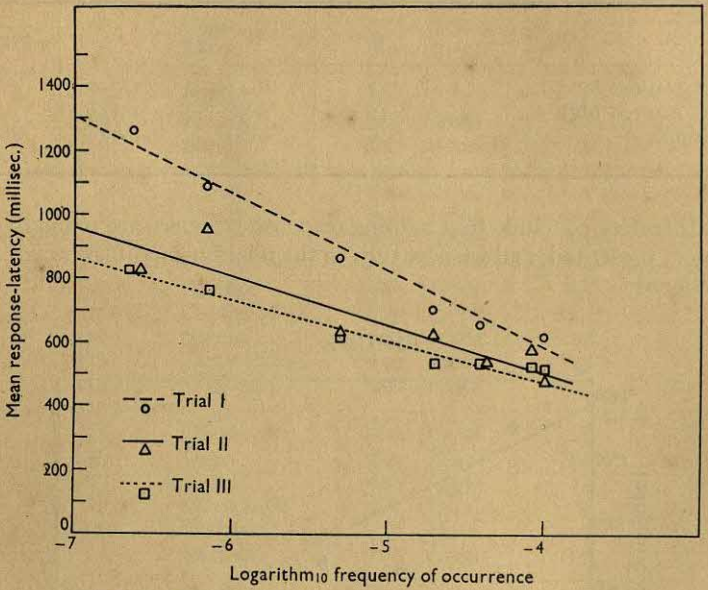


Response-latency to naming, for two sets of pictures and two groups of subjects, as a function of the logarithm_{10} of the frequency of occurrence of the object-name in print

It should first be explained that the Thorndike–Lorge Word List does not discriminate word frequencies between 50 and 100 per million words of text, nor does it do so for any frequencies above the latter value. All words in the first class are entered as A and all in the second as AA. The naming-latencies obtained in Series I and Series II are plotted in Figure 3, with the stimulus objects grouped into seven classes according to their frequencies. It can be seen that a linear relationship subsists between latency and the logarithm of word-frequency. Analysis of variance confirms the fit of the line. The product-moment correlation between log frequency and latency for Series II ($n = 26$) is $-0{\cdot}80$, $t = 6{\cdot}53$, $p \leqslant 0{\cdot}001$, while for Series I, $r = -0{\cdot}89$, $t = 9{\cdot}56$, $p \leqslant 0{\cdot}001$. The correlation between latencies of Series I and Series II is $r = 0{\cdot}74$, $t = 5{\cdot}40$, $p \leqslant 0{\cdot}001$. An analysis of variance fails to show the differences in latencies between Series I and II to be significant.

The effects of small amounts of practice (up to three trials) are also conveniently demonstrated in terms of the latency-frequency relationship. Figure 4 shows the points corresponding to those plotted in Figure 3. It would appear that, while some considerable improvement—amounting to about 350 millisec. for the least common names and about 100 millisec. for the most common—takes place between the first and second trials, much less (virtually none at the higher-frequency end) is occasioned by the second repetition.

FIGURE 4



Effect of practice on response-latencies to naming

CONCLUSIONS

It emerges from the experiments described that the psychophysiological process by which the name of an object is evoked occupies a longer time when the object and its name are of rare occurrence than when they are ones commonly encountered. It might be suggested that one reason for this lies in the known tendency for words of high frequency to be shorter than those of low, since presumably a greater time is needed to prepare the motor patterns of articulation for long than for short words. Our own data, however, do not bear out this suggestion. Partial product-moment correlations between log frequency and latency when, first, number of syllables and, secondly, number of letters in the name are held constant are $-0{\cdot}618$, $t = 3{\cdot}77$ ($p < 0{\cdot}001$) and $-0{\cdot}636$, $t = 3{\cdot}95$ ($p < 0{\cdot}001$) respectively. With log frequency held constant, on the other hand, corresponding coefficients between latency and name-length are $+0{\cdot}200$, $t = 0{\cdot}978$ ($p > 0{\cdot}3$) and $+0{\cdot}268$, $t = 1{\cdot}33$ ($p > 0{\cdot}1$).

The fact that the relationship between latency and log frequency is a linear one (so far as our present data go) is interesting, though we are not in a position to advance any explanation of the linearity as such, nor do we wish to base any specific conclusion on it. But it seems reasonable to conclude that since this empirical relationship is *not* a line parallel to the X-axis (constant latency for all word-frequencies), the cerebral storage mechanisms involved in object naming are organized according to a classificatory system and that access is gained to items in the store by the use of a "key." Since the higher the frequency the lower is the latency, we further conclude that the store is so arranged that the more frequently required items are the most quickly and easily accessible. Such a conclusion would be in keeping with the results obtained by Rochford and Williams (1962) in experiments on the different degrees of difficulty presented by different objects when dysphasic patients*, children and normal adults distracted by other concurrent stimuli, are called upon to name them.

We wish to express our gratitude to the Medical Research Council for the financial support which made this work possible. We are also indebted to Mr. B. Babington-Smith and Mr. J. F. Scott for statistical advice, and the Editor of *Nature* for allowing us to reproduce Figure 3 in this present communication.

*Our series of pictures is at present being routinely used in testing dysphasic and other patients with organic cerebral conditions by Mrs. F. Newcombe in the Department of Neurology, Churchill Hospital, Oxford. A preliminary report of the results of this work is given in Newcombe, Oldfield and Wingfield (1964).

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TEMPORAL FACTORS IN PATTERN VISION

BY

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A sequence of uncorrelated randomly patterned visual stimuli ("visual noise") is normally seen as a field of particles in "Brownian motion." When each frame of the sequence is followed by a blank flash superimposed on the same region of the visual field, the apparent structure of the noise field is strikingly altered, its form varying with the time interval between frame and flash. At a critical interval, many dots seem to cohere, to form maggot-like objects.

Some of the factors determining this critical interval have been studied. They include the brightness, repetition frequency and exposure duration of the noise field, and the distance of its retinal image from the fovea.

The critical interval for "perceptual blanking" is quite different from that for the "maggot effect," but the two show a suggestively similar dependence upon the duty cycle of the noise display.

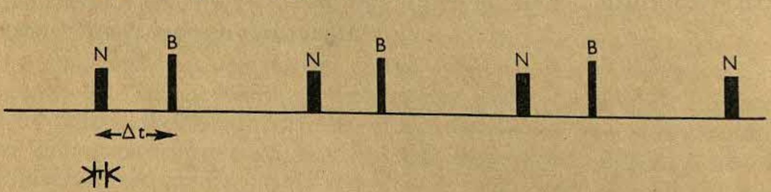
It is of some neurological interest that the phenomenon is not appreciably visible with dichoptic mixing of noise and blank stimuli.

INTRODUCTION

Spatiotemporally random optical fields ("visual noise") have many applications in the study of visual mechanisms (MacKay, 1957, 1961, 1965). As with a temporal noise signal applied to an electrical network, visual noise can show up various kinds of "bias" in the information-processing system. This paper is concerned with a particular kind of perceptual bias which is dependent on the temporal relations of the light signals entering the eye.*

When successive images presented to the eye are sufficiently random in form and uncorrelated in temporal sequence, what is normally seen is a mass of "particles" in apparent Brownian motion. There is considerable evidence (MacKay, 1961, 1965) that what is perceived is an internally generated synthetic approximation to the input, resembling it in certain overall properties such as density of texture, grain size, and the like, but manifesting a number of non-random structural features which are visibly locked to the retinal frame of reference.

FIGURE 1



Time relationships of stimuli used. N: Random noise presentation. B: Blank flash.

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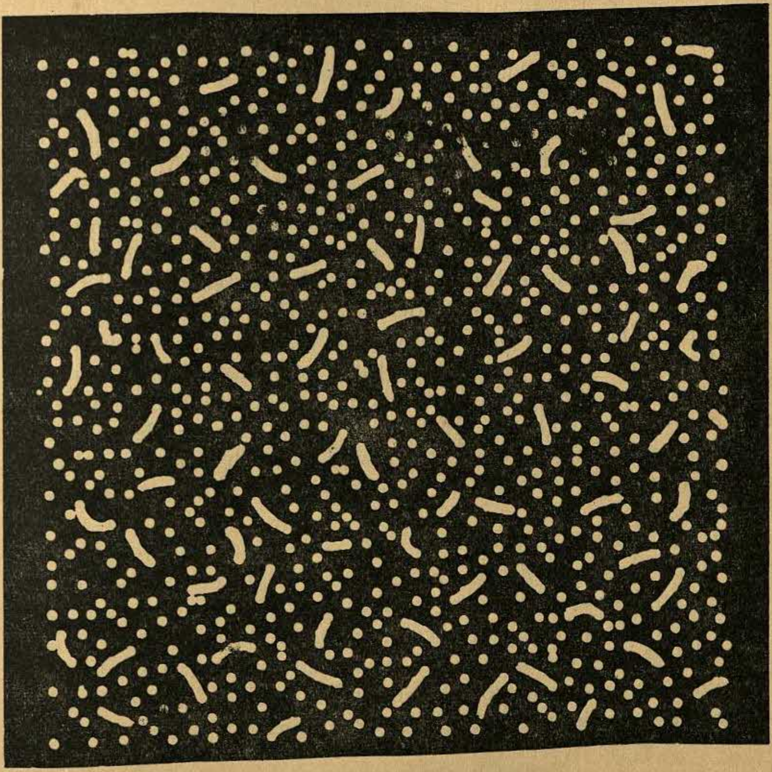
* This effect was demonstrated (by D. M. M.) at the Cambridge meeting of the Experimental Psychology Society on 3rd January, 1962. The present paper was given at the Oxford meeting of this Society on 10th July, 1964.

If such a repetitive sequence of random noise stimuli is interlaced with blank flashes at a slightly higher or lower frequency, a remarkable restructuring of the perceived field is observed by all subjects so far tested: Each time the delay $\triangle t$ of the blank flash B (Fig. 1) relative to the noise stimulus N reaches a certain critical value, the perceived field undergoes violent agitation, becoming populated with wriggling elongated objects resembling maggots (Fig. 2) which sometimes cohere to form a network of even longer "worms". The effect is slightly stronger if the frequency of blank flashes is lower than that of the noise stimuli—i.e. if the blanks come progressively later in the stimulus cycle; but it is clearly visible (over a certain range of frequency—see below) even if the delay $\triangle t$ between noise and blank is stationary at the critical value.

FIGURE 2



A (static) impression of the appearance of the screen under the critical condition for "maggots." The actual pattern on the screen consisted entirely of randomly distributed white dots. (It was agreed by all of ten subjects that this is a fair sample representation of the effect.)

It is not essential that the interlaced stimuli B be blank. A second source of random noise sequences can be used with somewhat similar effects. Even with a stationary noise pattern some perceptual anomalies are observable at the critical phase, white and black areas of the field appearing to move irregularly relative to one another. The white areas also flicker noticeably, but the black do not. The interpolation of the blank flashes seems here to inhibit the perception of single spots in favour of the formation of groups of spots into coherent areas. In all cases the optimal interval is well-defined and has much the same value, when allowance is made for exposure duration and brightness. It would seem therefore that the critical factor is the rhythm of the optical signals. On the other hand, the same rhythm with all-blank (i.e. unpatterned) signals has been found to give no similar perceptual effects.

Two points of some theoretical interest emerge from comparisons of monocular, binocular and dichoptic viewing conditions. In the first place, the effect is much stronger with binocular than with monocular viewing. Both the degree of agitation and the distinctness of the forms seen are enhanced. This contrasts in one respect with the viewing of a simple noise sequence, which gives a stronger impression of agitation monocularly than binocularly (MacKay, 1961).

Secondly, dichoptic presentation of noise and blank flashes (i.e. N in one eye and B in the other) has no comparable effect. This and the foregoing observation are consistent with the hypothesis that the neural activity responsible is on the peripheral side of the binocular fusion mechanism.

To complete this qualitative introduction it may be mentioned that at other values of delay Δt, interpolation of blank flashes B has various effects distinct from those under discussion. (*a*) Placed a few millisec. after each noise exposure (occurring, say, 10 times per sec.) it produces the familiar "perceptual blanking" (Baxt, 1871), which dims or even obliterates the noise pattern seen. (*b*) At about 20–30 millisec. interval (see below) we have the phenomena which are the subject of this paper. (*c*) With the blank flash B at still later points in the cycle the field returns towards its normal appearance, but gives the impression of greater cohesion between the objects seen in it, rather as if some of the spaces formerly between dots of the pattern had turned black. (*d*) At delays of the order of a half-period or greater, the field may resume more or less its normal form, but with very bright flashes structures resembling the familiar "stroboscopic patterns" (Smythies, 1959, 1960) may be seen.

The apparent luminance of the combined field fluctuates according to the value of Δt (though curiously enough this fluctuation is not measurable with a photometer (S.E.I.) using a small comparison spot). It was noted that the critical value of Δt for the "maggot" effect did not coincide with either a maximum or a minimum of the subjective intensity.

Wilson (1960) reported that when a train of blank flashes was interlaced with a sequence of flash-exposures of a stationary black-white boundary, an impression of motion of the boundary was created at intervals between figure presentation and blank flash of approximately 30 and 70 per cent. of the repetition cycle. At the values of Δt for which this illusion was maximally visible with a steady boundary, however, we have not found anything noteworthy with noise figures. The optimal interval for Wilson's effect appears to be greater by several millisec.

Apparatus

Two alternative types of visual noise source were used in this investigation. The first was a closed 3 ft. loop of cine film of random arrays of dots, in random temporal sequence, projected at 5–20 frames per sec. from a 16 mm. 500 W. projector. The latter was modified by the removal of the rotating shutter (which normally gives 3 exposures of each frame) and the addition of an external shutter rotating in the light path between lens and screen at the frame frequency. The aperture of this shutter and of the projector lens could be varied, the film being transported during the "off" phase.

On the same shaft as the shutter a cam was mounted so as to close a contact once per revolution and fire a Dawe Stroboflood lamp directed at the projection screen. The contact was mounted on a perspex disc with slip rings, so that the delay between film exposure and flash could be continuously varied either by hand or by a reversible electric motor. A further contact on the disc could be used to produce an audible click at any point in its revolution, to enable subjects to indicate the position at which the "maggot" effect was strongest when the disc was continuously motor-driven.

The alternative noise source comprised a turntable carrying a static noise pattern and rotating at about 15 revs./sec. (the exact speed being unimportant) asynchronously with an EMI stroboscope (type 4) which illuminated the "noise" in a succession of unrelated orientations. With the help of a half-silvered mirror, the image of a second similar stroboscope (covered with a diffusing paper) could be optically superimposed on that of the turntable to provide the blank flash. When required, the second stroboscope (minus diffusing paper) could be directed on the turntable itself in parallel with the first but at a variable phase. In later experiments two Dawe Stroboflood lamps (Model 1201E) were used, triggered by a multiple-pulse electronic timer.

A photoelectric monitor cell and calibrated oscilloscope enabled all necessary time relationships to be measured. With the first apparatus, the stroboscopically "frozen" image of the rotating shutter could be observed against a calibrated circular scale in order to measure the delay between beam cutoff and the blank flash from the stroboflood.

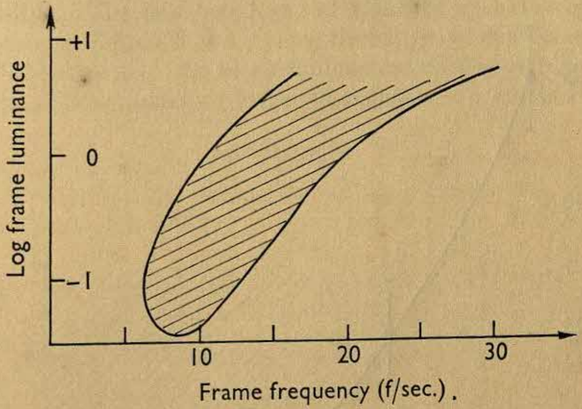
RESULTS

The measurements that follow were made with the two authors as subjects. Little systematic variation was found between other subjects in preliminary trials, or between the authors in these runs.

(*a*) *Effect of average luminance of the noise field. Negligible exposure duration. Stationary delay between noise stimulus and blank flash.*

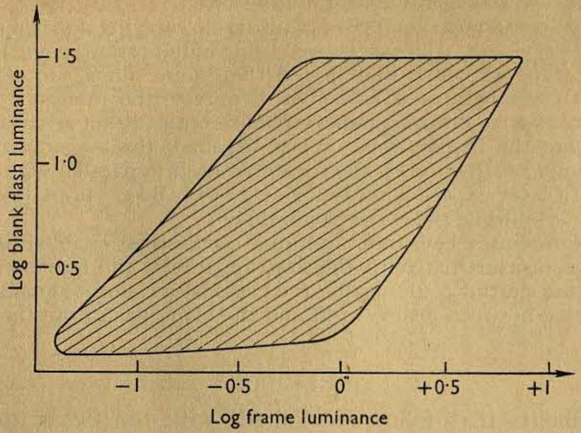
The average luminance of the noise field was found to determine the range of frequency and intensity of the blank flash over which the "maggots" were visible. The shaded areas in Figures 3 and 4 represent typical ranges when the duration of the noise exposure was negligible (i.e. when the second noise source was used), with a stationary delay $\triangle t$ between stimulus and blank flash. The average subjective luminance of the noise field at a rate of 18 f.p.s. was measured by comparison with a calibrated steadily illuminated spot (S.E.I. photometer), the subjective luminance being considered a more relevant parameter than objective flash intensity in this connection. The average blank field luminance was measured by the same method. In the graphs all luminances are expressed in log footlamberts.

FIGURE 3



Frequency range of visibility as a function of frame luminance. (Luminance of blank flash adjusted for optimum visibility at each point.)

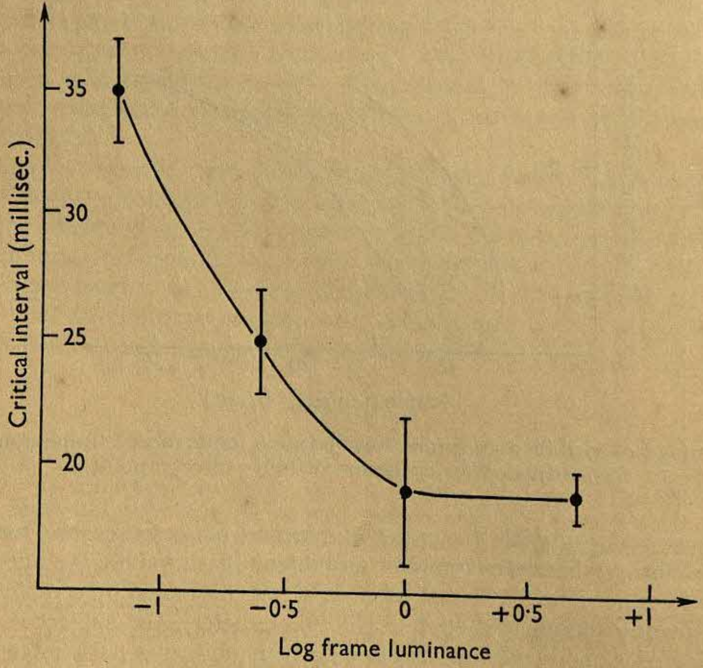
FIGURE 4



Limiting combinations of frame and blank luminance values for which "maggots" were visible.

The critical interval for the visibility of "maggots" at 18 f.p.s. varied with the average noise luminance as shown in Figure 5, being of the order of 18 millisec. at the highest luminances. It seemed to be independent of blank flash intensity (which was usually about 1 log unit higher) and of stimulus frequency, within the whole range over which the "maggots" were visible for a given noise luminance. For example, the

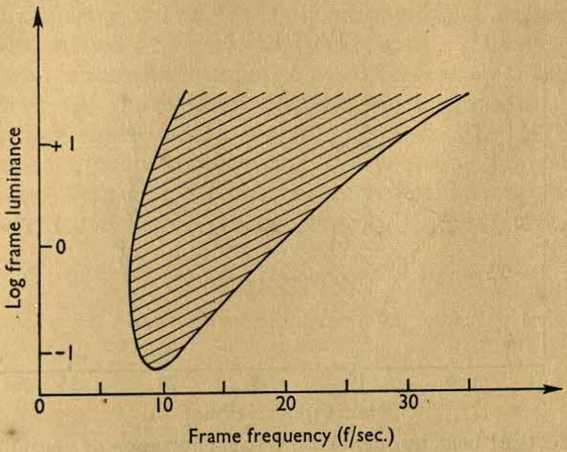
FIGURE 5



Effect of frame luminance on critical interval at 18 f.p.s. (Blank luminance adjusted for optimum visibility.)

critical interval $\triangle t$ for a noise luminance of 0 log ft. L proved to be 19 ± 3 millisec. for any blank field luminance between 0·2 and 1·5 log ft. L, while for a frame luminance of −1·2 log ft. L the critical interval was 35 ± 2 millisec. for blank field luminances of 0·1 and 0·4 log ft. L, no "maggots" being visible outside this range.

FIGURE 6

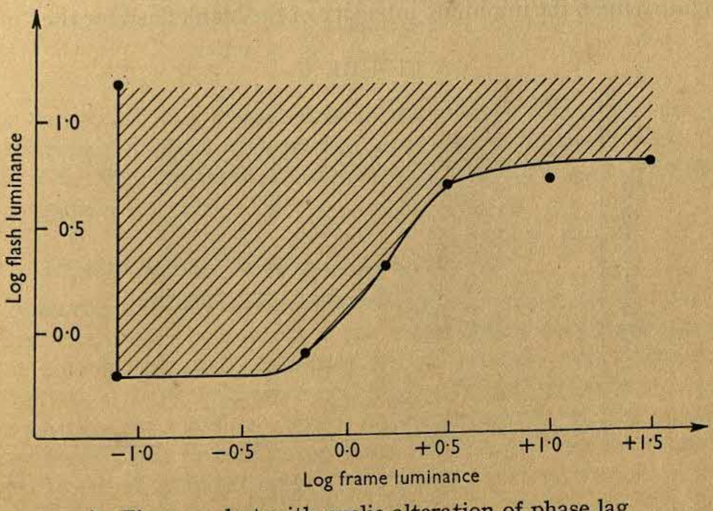


Frequency range of visibility as in Figure 3, but with cyclic alteration of phase lag.

(*b*) *Effect of beat frequency* (*negligible exposure duration*)

When the phase of the blank flash is altered cyclically, the rate at which it is altered affects the range of frequency and of blank flash intensity over which the "maggots" are visible. The best conditions of visibility are obtained when the cycle repeats every few sec. Figures 6 and 7 show the ranges of visibility recorded with a cycle duration of about 10 sec., the blank luminances for Figure 6 being those represented in Figure 7. These ranges are larger than those for a stationary delay (Figs. 3

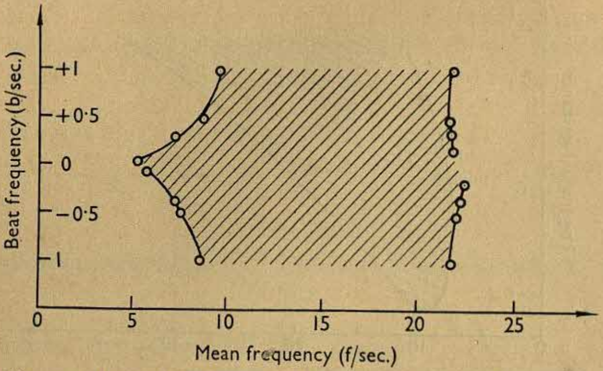
FIGURE 7



As Figure 4, but with cyclic alteration of phase lag.

and 4), and the upper limit of blank luminance was not in fact found, as "maggots" were still visible at the highest intensity available in that experiment. Figure 8 shows how the range of frequency for a fixed noise luminance (—0·3 log ft. L) varied with the beat frequency in a typical case. Although the "maggot" effect appeared sharper when the blank frequency was lower than that of the noise source, it will be seen that the frequency range of visibility was not significantly increased.

FIGURE 8



Effects of beat frequency on frequency range of visibility.

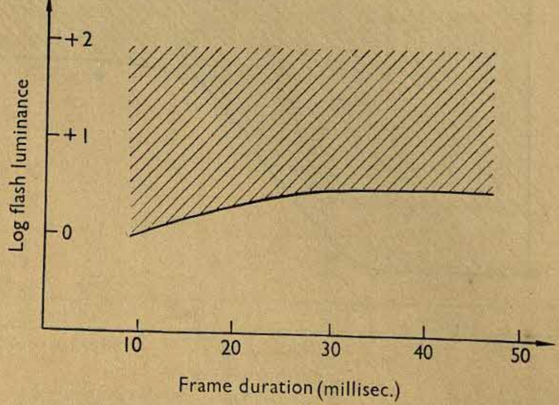
(*c*) *Effects of spot size*

Changing the average spot size of the noise source from 3 min. to 1 min. of arc somewhat narrowed the limits of visibility. The "maggots" in each case were to scale, having a length several times the width of a noise spot (i.e. about 20 min.with the larger spot size). Their size did not seem to vary appreciably over the field of vision (±10 degrees). They were still visible in small numbers when the noise field was cut down to 1 degree in diameter.

(*d*) *Effects of exposure duration on minimum intensity of blank flash*

Varying the exposure duration τ (Fig. 1) between 9 and 47 millisec. had the effect shown in Figure 9 upon the minimum intensity of the blank flash for the "maggots" to

FIGURE 9



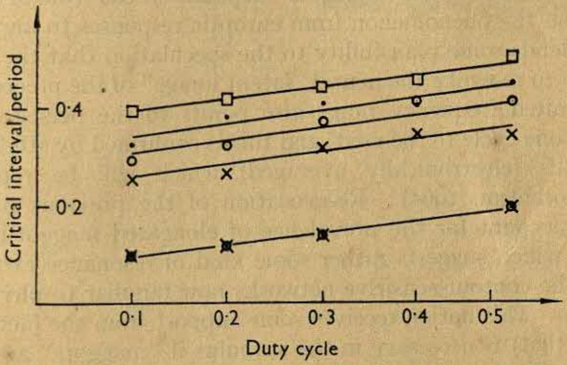
Effect of frame duration upon minimum intensity of blank flash.

be seen. It will be noted that at durations above some 30 millisec. the minimum intensity remains constant, suggesting that this is the time required for complete integration of the optical information in a single frame, at the neural level where the frame and flash signals interact.

(*e*) *Effects of exposure duration on critical intervals*

Using the film projector, the effects of exposure duration τ upon critical intervals $\triangle t$ from onset were studied at various projection rates. Changes in the total light output per exposure with rate were annulled by compensatory changes in the aperture of the projector lens. As the exposure duration was increased, a linear increase was observed in the critical interval expressed as a fraction of the period T, both for the "maggot" effect and for maximum blanking, For blanking, $\triangle t/\mathrm{T}$ was independent of frequency in the range 8–13 c.p.s. The fractional critical interval $\triangle t/\mathrm{T}$ for "maggots," however, was greater for higher frequencies (Fig. 10) It was found that if the critical interval for "maggots" was measured from the point in the cycle corresponding to maximum blanking (instead of from the onset of the noise stimulus) this interval turned out to be fairly constant (22 millisec.) for any duty cycle or frequency in the ranges investigated.

FIGURE 10



Fractional critical interval $\Delta t/\mathrm{T}$ for blanking (lowest line) and for "maggot" effect (upper four lines) as a function of exposure duty cycle, τ/T.

□ : 900/min.
● : 700/min.
○ : 600/min.
X : 500/min.

(*f*) *Effect of distance from the fovea on critical interval.*

It was observed that with a continuously variable delay $\triangle t$ the area of agitation seemed to move outwards or inwards in the visual field. Accordingly, using the film projector at 10 exposures/sec, values of critical interval were obtained when the central 6° and 12° of the field were occluded. The critical interval was found to increase on average by 4 millisec. and 5 millisec. respectively, the values being the same with either a black or a diffusing central region. When the occluding surface was arranged to cut off only the blank flash (not the noise) from the central region of the field, no spread of the "maggot" effect into the central region was observed.

Discussion

Most of these results speak for themselves, and where they do not we can do little more than indicate the questions they raise.

That the phenomenon requires the two flashes to be roughly matched in brightness (Figs. 3, 4, 6, 7) is not surprising. The variation in critical interval with frame brightness is more interesting, as inviting comparison with physiological measurements on the latency of evoked responses as a function of brightness.

The initial slope in Figure 5 does agree roughly with the rate (10 millisec. per log unit) at which luminance is known to affect physiological latency. On the other hand, the relative insensitivity to the luminance of the second (blank) flash is puzzling, unless we assume that at all blank luminances for which "maggots" are produced the appropriate visual mechanism is effectively saturated and responds to the blank flash with minimal latency. The alternative interpretation, that the interaction is peripheral to the signalling system responsible for the physiological latency, would require us to explain why the critical interval should be affected by the luminance of the noise frame.

The fact that the scale of the "maggots" depends on the scale of the noise pattern helps to distinguish the phenomenon from entoptic responses to unpatterned stroboscopic light, and lends some plausibility to the speculation that the blank flash may serve in some way to re-evoke the neural "latent image" of the preceding noise frame. The relatively limited frequency range also points to the persistence of response-components from one cycle to the next, and this is confirmed by studies of the evoked occipital potentials (electronically averaged) which will be reported elsewhere (MacKay and Fiorentini, 1965). Re-evocation of the previous pattern, however, would not alone account for the prevalence of elongated maggot-like forms in the combined image, which suggests rather some kind of resonance evoked by properly timed signals in the contour-sensitive networks now familiar to physiologists (Hubel and Wiesel, 1961). This notion receives some support from the fact that contouring (and irregular at that) is necessary in the stimulus if "maggots" are to be seen, and that even when the contours are stationary from frame to frame, some traces of worm-like structures are observed among them under the conditions which would produce "maggots" with a moving noise field. A similar "resonance" in velocity-sensitive networks might account for Wilson's (1960) motion illusion referred to in section 1.

Although the "maggot" effect clearly occurs at a different phase from perceptual blanking, and is phenomenally quite distinct, an interesting connection emerges in terms of Fig. 10, where the lowest graph represents the critical interval for blanking as a fraction of the period. The identity of slope between the various graphs shows that what matters (regardless of duty cycle) is the delay of the second flash measured from the critical point for perceptual blanking. This suggests (but no more) the possibility that perceptual blanking and the "maggot" effect may involve two phases of operation of the same neural mechanism.

Special interest from a neurological standpoint attaches to the observation that binocular viewing gives much stronger effects than monocular, and that dichoptic mixing of noise and blank flashes gives none. It has suggested an investigation of retinal and cortical evoked potentials under these various viewing conditions which we have begun and will also report elsewhere (MacKay and Fiorentini, 1965). Here it need only be said that specific correlates of the "maggot" effect have indeed been observed in the evoked occipital potentials and are not present with monocular viewing.

Finally it should be pointed out that although the "maggot" effect is perhaps the most striking result of interpolating blank flashes into a train of visual noise frames, the other anomalous effects observed at different phases are equally challenging, and may well supply essential clues to a satisfactory theory.

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INCIDENTAL CUE LEARNING IN RATS

BY

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Continuity theories of discrimination learning appear to say that animals learn equally about all cues impinging upon their receptors; noncontinuity theories that they learn about only one cue at a time. Experiment I showed that neither of these positions is correct: rats trained to attend to one cue learned less about a subsequently introduced incidental cue than rats given no such pretraining; but attention to one cue did not totally prevent learning about the other. A second experiment established that if rats are trained with one cue, and a second cue is then also made relevant, the amount learned about this second cue varies directly with (*a*) the abruptness with which it is introduced, and (*b*) the difficulty of the original discrimination.

INTRODUCTION

Lashley (1942) trained rats on a size discrimination, subsequently making a second (shape) cue relevant, and finally gave several tests of how much the rats had learned about this second, incidental cue. He claimed that they learned nothing, and took this result to refute a continuity view of discrimination learning according to which all stimuli impinging on an animal's receptors are associated with responses. But the tests he used were not very sensitive, and consequently may have failed to measure such learning as occurred. Later experiments utilizing a similar design (Blum and Blum, 1949; Bitterman and Coate, 1950; Hughes and North, 1959) have all shown that rats do learn something about an incidental cue when it is correlated (fully or partially) with the primary cue in terms of which the original discrimination was established. This result was taken to refute a noncontinuity view of discrimination learning according to which animals attend to, and thus learn about only one cue at a time.

But this series of experiments leaves a number of questions unanswered. Animals might still attend to some cues more than to others: in the above situation, although they might learn something about the incidental cue, they might have learned still more about it had it not been incidental—i.e. had they not been given prior training with the primary cue. Sutherland, Mackintosh and Mackintosh (1965) have shown this to hold for octupuses, and the first experiment reported here was designed to test this prediction in rats. Secondly, if attention does modify the amount learned about a given cue, it should be possible to discover variables that affect the range of attention and therefore influence such incidental learning. Bruner, Matter and Papanek (1955) have shown that overtraining and high drive both decrease incidental learning in rats, and the second of the present experiments investigates two further possible variables.

EXPERIMENT I

Subjects and apparatus

The subjects were 48 experimentally naïve male hooded rats from the colony maintained at the Institute of Experimental Psychology, Oxford. They were 3–4 months old at the beginning of the experiment. The apparatus was a modified Lashley jumping stand, with platforms below each window, and is described in more detail elsewhere (Mackintosh, 1963).

Procedure

Subjects were placed on a feeding schedule, adapted to the apparatus, and pretrained to jump first to open and then to closed doors. Throughout the experiment, 20 trials were given daily with approximately 6–8 min. inter-trial interval. Training was by a modified correction procedure: after an incorrect jump, the subject was detained in front of the locked negative door for 10 sec., then replaced on the jumping platform and allowed a further two free jumps. After three incorrect jumps, the fourth was forced to the correct side. Correct jumps were rewarded with 20 sec. access to food behind the doors. For further details of training procedures, see Mackintosh (1963).

Experimental design

Stage 1. The two experimental groups (32 subjects) were trained on a successive brightness discrimination, to jump right to two black stimuli and left to two white stimuli. For Group BW (16 subjects) the stimuli were black and white Perspex squares, 20 cm.^{2} in area. For Group BhvWhv (16 subjects) the stimuli were black and white 10 × 2 cm. rectangles. For both groups on all trials the stimuli were either both black or both white. For Group BhvWhv the rectangles were orientated either both horizontally or both vertically: the orientation of the rectangles, however, was randomly correlated with reward. Thus Group BW learned a simple successive brightness discrimination, and Group BhvWhv learned a successive brightness discrimination in which the orientation of the stimuli was irrelevant. Training was continued for 240 trials. A Control Group of 16 subjects received no training at this stage.

Stage 2. All three groups learned a combined brightness and orientation simultaneous discrimination; half of each group was trained with a black horizontal (10 × 2 cm.) rectangle positive, and a white vertical rectangle negative; half was trained with a white horizontal rectangle positive, and a black vertical negative. Training continued for 60 trials; over the last 20 trials, all groups were performing at a 90 per cent. level of accuracy.

Stage 3. On each of four days, subjects were given 10 retraining trials on the stimuli of Stage 2, alternating with 10 transfer trials on which all jumps were rewarded. Two pairs of transfer stimuli were shown: (*a*) a black and a white 20 cm.^{2} square, and (*b*) a horizontal and a vertical gray 10 × 2 cm. rectangle. Performance on transfer pair (*a*) indicates the amount learned about the brightness cue during Stage 2, and performance on transfer pair (*b*) indicates the amount learned about the orientation cue during Stage 2. The order of trials was such that for each subject the "positive" transfer stimulus within each pair appeared equally often on the left and on the right, and appeared equally often on the same side and on the opposite side from the positive training stimulus in the preceding retraining trial.

Results and discussion

The hypothesis to be tested is that rats will learn less about a given cue if they are given pretraining designed to make that cue incidental. In the present context, it is predicted that the two experimental groups, having been pretrained to attend to brightness cues during Stage 1, should be less likely than the Control Group to attend to the orientation cue in Stage 2, and should therefore perform less accurately on transfer pair (*b*) in Stage 3. The results are shown in Table I. An analysis of variance on the scores of transfer test (*b*) showed a highly significant group effect ($F = 38{\cdot}20$, $d.f. = 2, 42$, $p < 0{\cdot}001$); thus it is clear that the successive pretraining procedure markedly reduced the amount learned about orientation. Further analyses showed that Group BhvWhv learned significantly less than Group BW ($F = 30{\cdot}99$, $d.f. = 1, 28$, $p < 0{\cdot}001$); and that Group BW learned significantly less than the Control Group ($F = 5{\cdot}22$, $d.f. = 1, 28$, $p < 0{\cdot}05$). Thus pretraining with orientation irrelevant reduced the amount learned about orientation almost to zero (although 13 out of the 16 subjects in this group scored above chance on the transfer tests, only one subject did so significantly, and a χ^2 test performed on the scores of the whole group fell far short of significance: $\chi^2 = 11{\cdot}70$, *d.f.* = 15, $p > 0{\cdot}70$). Pretraining with BW alone, although it did not prevent a significant amount of orientation learning from occurring ($\chi^2 = 83{\cdot}30$, *d.f.* = 15, $p < 0{\cdot}001$) did significantly reduce the amount learned about this cue.

TABLE I

RESULTS OF EXPERIMENT I

	Stage 1	*Stage* 2	*Stage* 3		
			Percent correct		
Groups	*Total initial errors*	*Total initial errors*	*Retraining trials*	*Orientation transfer*	*Brightness transfer*
Control	—	15·62	94·53	92·50	87·81
BW	60·94	12·37	93·90	83·12	85·94
BhvWhv	65·19	11·87	91·72	60·31	90·62

It is also worth noting that the training scores for Stage 2 show an effect similar to Lawrence's acquired distinctiveness of cues (Lawrence, 1949). Although all three groups reached the same level of performance by Day 3, the total number of initial errors over the 60 trials was significantly less for the Experimental Groups than for the Control Group ($F = 3{\cdot}33$, *d.f.* = 2, 42, $p < 0{\cdot}05$). That is to say, subjects pretrained (successively) to attend to brightness solved a subsequent (simultaneous) discrimination in which brightness was a relevant cue faster than a control group. This difference did not, however, last long: it had vanished by the last day of training, and, as can be seen, did not appear in the scores of the three groups on the brightness transfer tests.

The results of this experiment may reasonably be taken as a refutation of an extreme continuity position: they show that animals do not learn equally about all cues impinging upon their receptors. Conversely the fact that both experimental groups (and Group BW significantly) scored above chance on the orientation transfer tests, seems to provide further disproof of an extreme noncontinuity position: animals do not learn about only one cue at a time. But taken with some other recent results (Sutherland and Mackintosh, 1964), they support a compromise position that animals learn about some cues more than others, or that there is an inverse relationship between the probability of learning about one cue in a given situation and learning about another. In terms of a model of discrimination learning put forward by Sutherland (1959), animals do not utilize only one analyser at a time, but the more firmly one analyser is switched in, the less likely they are to utilize other analysers.

EXPERIMENT II

The results of Experiment I establish that rats do not equally utilize all analysers simultaneously, but they provide little information on what determines the probability of switching in an analyser. Furthermore, there is one ambiguity in the results not yet mentioned. Group BhvWhv learned significantly less about orientation than

Group BW. The most obvious interpretation of this finding is to say that Group BhvWhv had been trained precisely to ignore orientation differences during Stage 1, while Group BW had received no such special training. But there is an alternative possibility: the stimuli shown to Group BW were abruptly changed in shape between Stages 1 and 2; in Stage 1 this group was jumping to squares, in Stage 2 to rectangles. It seems quite likely that such an abrupt change along a given dimension will increase the probability of the appropriate analyser being switched in. Since Group BhvWhv was trained with rectangles during both Stages 1 and 2, the subjects may have failed to switch in an orientation analyser during Stage 2, either because they were not exposed to an abrupt change of stimuli, or because they were trained with orientation irrelevant, or for a combination of these two reasons. It does not seem possible to decide between these alternatives in this experimental situation, but it should be possible independently to investigate the effect of the abruptness of introduction of an incidental cue on the amount learned about it, and Experiment II was designed to do this. Rats were trained to criterion on an orientation discrimination between gray horizontal and vertical rectangles; for one group (Abrupt) the brightness of the rectangles was then changed abruptly to black and white, for the second group (Gradual) the change involved several intermediate steps during which one rectangle was made progressively darker, and the other progressively lighter. After further training on the combined brightness and orientation discrimination, subjects were finally tested for the amount learned about brightness: it was predicted that the abrupt group would learn less.

A preliminary experiment of this design, in which four intermediate steps were interposed for the gradual group, produced results that, although in the predicted direction, fell short of significance. Accordingly in the present experiment eight intermediate steps were provided. The preliminary experiment produced a second result which was followed up here: animals trained with their preference in the first stage of the experiment appeared to learn less about a subsequently introduced incidental cue, than animals trained against their preference. One possible reason for this may be that in general the easier the original problem the less will be learned about a second cue, and this possibility was directly tested in the present experiment by training animals in the first stage either on an easy horizontal-vertical discrimination or on a more difficult one.

Subjects, apparatus and procedure

The subjects were 48 naïve female rats, 3 months old, from the same stock as those in the previous experiment. The apparatus and training procedure were the same as before. During Stage 3, latencies were recorded with a stop-watch to the nearest second.

Experimental design

Thirty two subjects were used in the main experiment, and 16 in a Control Group discussed below.

Stage 1. All 32 subjects learned a horizontal-vertical discrimination to criterion (18 correct out of any 20 consecutive trials), and were given 20 overtraining trials. For half the subjects (easy discrimination) the stimuli were 10 × 2 cm. gray rectangles; for the other half (difficult discrimination) the rectangles measured 6·66 × 3 cm. Half of each subgroup was trained with horizontal positive, half with vertical positive.

The stimuli were cut from cardboard and painted mid-gray by mixing together black and white matt paints. They were mounted on ¼ in. thick wooden blocks which were glued centrally to the brown hardboard doors. The stimuli were thus effectively stereometric, as were the Perspex stimuli of the previous experiment, although in the present case the wooden blocks were cut smaller than the cardboard stimuli (8 × 1 cm. and 5 × 2 cm. respectively) so that their edges could not be seen.

Stage 2. Group A (16 subjects) continued Stage 1 training for a further 40 trials. Group G was shifted through eight intermediate steps, with each step lasting for five trials. The steps consisted of progressively darkening S+ and lightening S− for all subjects.

Stage 3. Both groups were now trained on a combined brightness and orientation discrimination for 100 trials, with S+ black and S− white for all subjects.

Stage 4. All subjects received 20 retraining trials alternating with 20 transfer trials, 10 with a black and a white horizontal rectangle, 10 with a black and a white vertical rectangle. The size of the rectangles was either 10 × 2 cm. or 6·66 × 3 cm. depending on the size of the training stimuli shown to each subject.

Control group. Sixteen subjects were trained for 120 trials on a simultaneous brightness discrimination by a noncorrection procedure. The stimuli were those used in the transfer tests for the experimental groups: thus eight subjects were trained with 10 × 2 cm. rectangles, eight with 6·66 × 3 cm. rectangles, and within each group four subjects were trained with both rectangles horizontal, four with both rectangles vertical. For all subjects, black was positive.

This group provided data on two points. First on how much is learned by naïve animals about this brightness cue in 100 trials; secondly whether the brightness discrimination between the 10 × 2 rectangles was as easy as that between the 6·66 × 3 rectangles. Comparison between the experimental groups on the amount learned about brightness can only be valid if the brightness discrimination is equally easy for both.

Results and discussion

The results are shown in Table II. An analysis of variance performed on the trials to criterion scores of Stage 1 showed: (*a*) that the 10 × 2 discrimination was very much easier than the 6·66 × 3 discrimination ($F = 179{\cdot}05$, $d.f. = 1, 28$, $p < 0{\cdot}001$; there was in fact no overlap between the scores of the two groups); (*b*) that subjects trained with horizontal positive learned faster than subjects trained with vertical positive ($F = 12{\cdot}90$, $p < 0{\cdot}01$); and (*c*) that there was no difference between Group A and Group G ($F = < 1$).

TABLE II

Results of Experiment II

	Groups					
	Gradual	*Abrupt*	10 × 2 *rectangles*	6·66 × 3 *rectangles*	*Horizontal positive*	*Vertical positive*
Stage 1 Trials to criterion	131·06	133·06	88·06	176·06	120·25	143·87
Stage 4 Percent correct on transfer trials	65·94	75·94	54·06	87·81	67·19	74·69

The results of the transfer tests were as follows: (*a*) Subjects trained with the 10 × 2 cm. rectangles learned very much less about brightness during Stage 3 than did subjects trained with the 6·66 × 3 rectangles ($F = 123{\cdot}97$, $d.f. = 1, 28$, $p < 0{\cdot}001$; again there was no overlap between the scores of the two groups): (*b*) subjects trained with horizontal positive learned less than subjects trained with vertical positive ($F = 6.16$, $p < 0.05$); and (*c*) Group G learned less than Group A ($F = 10.88$, $p < 0.01$).

This final result clearly demonstrates that the abruptness of introduction of an incidental cue significantly affects the amount learned about that cue. Since the result seems unambiguous, it need not be discussed further in this section.

The finding that subjects trained with 10 × 2 rectangles or with horizontal positive learned less than subjects trained with 6·66 × 3 rectangles or with vertical positive, supports the suggestion made above that the easier the initial problem the less will be learned about a subsequently introduced incidental cue. There are, however, a number of indirect ways in which this effect may have been produced, and the remainder of this section will be devoted to a discussion of these possibly critical factors.

(*a*) *Discriminability of the incidental cue.* The results would be trivial if for some reason the incidental cue were actually more discriminable for subjects trained with the 6·66 × 3 rectangle than for subjects trained with the 10 × 2 rectangles. Data from the Control Group, given in Table III, rule out this possibility: neither in terms of level of accuracy reached after 100 training trials, nor in terms of the total number of errors made in 120 trials, was there any significant difference between the two groups. It is true that one cannot hope to prove the null hypothesis; but the fact that the difference between the experimental groups on the test scores was large and significant, and the control group difference small and not significant, implies that the latter is not responsible for the former.

TABLE III

EXPERIMENT II: CONTROL GROUP RESULTS

	Groups	
	10 × 2 *rectangles*	6·66 × 3 *rectangles*
Trials 1–120 Total errors	48·50	49·50
Trials 101–120 Per cent. correct	80·62	78·12

(*b*) *Response latencies.* From the first row of Table IV, it can be seen that subjects trained either against their preference or with the more difficult rectangles responded more slowly over Trials 1–20 of Stage 3 (these differences decreased during the course of Stage 3 training). It is possible that the more slowly a subject responds, the more time it has in which to see the critical stimuli, and the more it will in consequence learn about a newly introduced cue. Since Group A also responded more slowly than Group G at this stage, it is possible that the results of the entire experiment are due to this factor.

To test this, an analysis of variance was performed on these latency data, but none of the differences reached acceptable levels of significance. The largest difference, that between subjects trained with horizontal and with vertical positive, only yielded an F of 3·40 ($d.f. = 1, 28$, $0.10 > p > 0.05$). Neither of the other two differences even reached the 0·10 level of significance. As a second test, rank-order correlations were run between the latency scores and the transfer test scores. If longer latencies provided greater opportunities for learning, there should be an inverse correlation between the two sets of scores. Since there were only four subjects per subgroup, it was hardly to be expected that any within-subgroup correlation would reach significance: but the general trend should be negative. The highest of the eight subgroup correlations was +0·80, and six out of eight were positive. Thus it is clear that within subgroups there was, if anything, a tendency for subjects with longer latencies to learn less about the incidental cue; and although the overall correlation for all subjects pooled together was negative ($\rho = -0 \cdot 14, p > 0 \cdot 40$), it seems extremely unlikely that differences in latencies were the *cause* of differences in the amount learned about incidental cues.

TABLE IV

EXPERIMENT II: GROUP MEANS OF THE MEDIAN LATENCY OF EACH SUBJECT OVER TRIALS 1–20 OF STAGE 3. TOTAL ERRORS DURING STAGE 3.

	Groups					
	Gradual	*Abrupt*	10 × 2 *rectangles*	6·66 × 3 *rectangles*	*Horizontal positive*	*Vertical positive*
Latencies in seconds	7·53	10·09	7·59	10·03	7·14	10·48
Errors	1·87	1·56	1·06	2·37	2·06	1·37

(*c*) *Number of errors.* If subjects continue to make errors on the orientation problem after reaching criterion (because of the difficulty of the discrimination), it is possible that they might increase their probability of reward by attending to the more discriminable brightness cue. This would appear to imply a relatively straightforward explanation of the role of problem difficulty; and, as can be seen from the second row of Table IV, subjects trained with the 6·66 × 3 rectangles did continue to make more errors during Stage 3 than subjects trained with the 10 × 2 rectangles ($F = 6 \cdot 40$, $d.f. = 1, 28$, $p < 0 \cdot 05$). On the other hand, the difference between subjects trained with horizontal and with vertical rectangles was in the opposite direction. Nor do the results of correlations run between these error scores and the transfer test scores provide any support for this suggestion: only four out of the eight within subgroup correlations were in the predicted negative direction: and the overall correlation was far short of significance ($\rho = -0 \cdot 18, p > 0 \cdot 30$).

Difficulty of original learning, therefore, does not seem to have had its effect on incidental learning via any of the indirect factors discussed here. The only measure that was significantly correlated with the transfer test scores was original learning speed ($\rho = -0 \cdot 80, p < 0 \cdot 001$).

GENERAL DISCUSSION

The present experiments have produced three main findings.

(*a*) Experiment I showed that if subjects are trained on a combined brightness and orientation discrimination, then the amount learned about one of these cues can be significantly reduced if subjects are pretrained to attend to (and hence solve the combined problem in terms of) the other cue. This is not an all-or-none matter: the subjects learned something about the incidental cue, but less than they would have done, had they received no pretraining. Thus the rat can be said to have some limited perceptual capacity: the more it attends to one cue, the less likely it is to attend to another. In terms of the account of discrimination learning put forward by Sutherland (1959), there is an inverse relationship between the probability of switching in different analysers.

(*b*) If animals do not automatically analyse all cues simultaneously, it becomes important to discover what causes them to utilize a given analyser in a given situation. In the present context: what factors affect the probability of incidental cue learning? Experiment II showed that the abruptness of introduction of the cue is one important variable. The more gradually an incidental cue is introduced, the less animals learn about it. The result is not entirely surprising, but it is still of interest, both in interpreting other experiments, and from a theoretical point of view. Terrace (1963) for example trained pigeons on a colour discrimination, added an incidental cue of orientation, and showed that the birds could continue to perform with 100 per cent. accuracy in the absence of the original colour cue. The results demonstrate even more clearly than any presented here that animals are capable of utilizing more than one analyser on any one trial. But it is worth noting that although Terrace faded out the colour cue gradually, he introduced the orientation cue abruptly. It now seems likely that this was a critical factor in producing the obtained results.

From a theoretical viewpoint, these results appear to present some problems. When an abrupt change was made in the brightness of the rectangles, subjects learned about the incidental brightness cue; but when a gradual change was made they were less likely to learn. To say that subjects learned about brightness, presupposes that they were using a brightness analyser; the results show therefore that an abrupt change in brightness increased the probability that animals would switch in a brightness analyser. But how could any change in brightness be detected unless subjects were already attending to brightness? The solution to this paradox would seem to be to distinguish two senses of "analysing" a given cue. Animals must be supposed always to detect stimulus differences, but only to "analyse," i.e. store information about, a small subset.

(*c*) The final result to be considered is the finding that the more difficult the original problem the greater the probability that animals will attend to a subsequently introduced incidental cue. MacCaslin, Wodinsky and Bitterman (1952), and Perkins, Hershberger and Weyant (1959) have suggested a comparable relationship between the difficulty of a discrimination and the slope of generalization gradients obtained between the original S+ and some new stimulus.

The claim made above that this effect of problem difficulty is not mediated indirectly via response latency or error rate, has implications for at least one account of attention. Broadbent (1958, 1961), has suggested that animals have a limited capacity for attending to different cues, and that this limit is imposed by their capacity for processing information. If therefore two groups were trained on different discriminations, one involving more information than the other, and if for each group an additional cue was added, then other things being equal the group with the more information capacity to spare (i.e. the one trained on the low information content problem) should be more likely to attend to the additional cue. Information theorists have, of course, suggested that the difficulty of a discrimination is one determinant of the amount of information involved (Crossman, 1955), and this suggestion has been explicitly endorsed by Broadbent in the present context (1961, pp. 267–8); thus

Broadbent clearly predicts that the easier the original problem, the more subjects will learn about an incidental cue; and the present results indicate some inadequacy in his account.

It is less easy to give an adequate explanation of these results. Perhaps they could be adduced as an instance of the law of least effort, and one might postulate that subjects will attend to whichever cue (*a*) is consistently correlated with reward, and (*b*) provides the easiest predictor of reward.

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THREE ESTIMATES OF THE WORD SPAN AND THEIR STABILITY OVER THE ADULT YEARS

BY

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Three estimates of the immediate word span were compared in groups of men representing each decade between the ages of 20 and 69 years. No systematic change with age was found in free recall or in partial recall requiring the reproduction of the unrepeated word in lists of four to seven items. When the instruction was to report the entire list but leave the unrepeated word last, performance steadily deteriorated with age. Mean success in recall decreased on three criteria, and error by false terminal placement increased from each decade to the next. The results support the hypothesis that capacity to perform simultaneously two such operations as sub-vocal rehearsal of one item and vocal enumeration of the other items shrinks with the advancing years. This interpretation of the findings agrees with other observations about age-related changes in perceptual-motor skills and cognitive capacity, and indicates the manner in which an increased propensity to forgetfulness with the advancing years originates in immediate memory. It is also noted that in the general population the immediate word span is well below that determined in college students, even under the least exacting conditions of free recall, and that the additional requirement to search for, select and test a required item does not further restrict the word span below the age of 50.

Memory tends to deteriorate with the increasing years, especially for recent information. Several circumstances may contribute to this change in cognitive capacity, and have been considered in accounting for it. Experiments, as well as uncontrolled observations, point to the process of registration as the critical stage in the chain of operations by which information is processed, recorded for future reference, stored, and retrieved. This view is not supported by tests of the standard digit span (Gilbert, 1941; Wechsler, 1955) which barely declines over the first five decades of adulthood, a finding that raises some questions of interest for general psychology as well as for gerontology.

The retention of short messages over a few seconds decreases quite notably with age (Griew and Davis, 1962; Broadbent and Heron, 1962; Inglis, 1965). This could happen because of an autonomous decay process which becomes faster over the years, or because susceptibility to interference increases. Evidence in support of the latter explanation has been furnished by experiments in serial response tasks, when the operator selects and registers signals for subsequent action while he is engaged in responding to signals received several steps earlier (Kay, 1953; Kirchner, 1958). The relevance of these experiments in motor skill to age changes in memory has been pointed out by Welford (1958) and by Kay (1959). Both authors stressed the importance of interference effects with the short-term retention of messages that exceed the immediate memory span. Kay proposed that the interference of the items in excess of the span with the message already held causes the failure of older persons. Other sources of interference could be traced to processing operations, when these have to be performed on the messages received or in their delivery. A special case in point arises when a part of the message is held while the remainder is being reported.

The purpose of the present experiment was to test the differential effects of three hypothetical sources of interference on the immediate word span at five chronological age levels. They were interference by recall; interference by such processing operations as scanning, selection and testing for fit; and a combination of these two sources of interference with the requirement to hold the one selected item while reporting the others.

METHOD

The experiment in immediate memory formed part of a battery in a study of ageing effects, occupying the middle of a 2-hr. session. The subjects were 200 healthy, normal men, with a high school education, and working in or recently retired from clerical jobs. They were equally distributed over the five decades between 20 and 69 years of age.

The messages to be memorized consisted of familiar monosyllabic English words, recorded on magnetic tape and played back at a level of comfortable intelligibility. No word was used in more than one string, and within each list rhyming, alliteration and other formal or meaningful patterning was avoided. The words were presented at the rate of one per sec. and, when repeated within a string, there was no gap between the two halves. Strings varied in length in a randomized order, and the end was marked by a short, soft buzzer. To minimize proactive interference exerted by previous lists on subsequent ones, the experiment was interleaved with a test in manual reaction time, so that a block of 10 or 20 trials in this test followed each attempt at recall.

According to the method of recall, the experiment divided into five sections, with two replications, as set out in Table I. Under each instruction recall was by vocal reproduction. Instruction for recall, given before each section, included an explanation of the composition of the strings but no indication about their length. Sections I and V tested *free* recall, i.e. the reproduction of all words listed or of as many as the subject remembered, in any order. These two sections consisted of seven strings, one in each numbering 4, 5, 6, 7, 9, 11 and 13 different words. Sections II and IV, and the two halves of Section III were each made up of four strings, 7, 9, 11, and 13 items in length. These were also compiled from unrelated words, except that all but one occurred once in the first and again in the second half of a string, but in a different order. The position of the unrepeated word was systematically varied in the first half of the strings, and no new word occurred in the second half.

TABLE I

EXPERIMENTAL SCHEDULE

Section	*Strings*	*String length*	*Different words (list size)*	*Instruction*	*Condition*
I	7	4–13	4–13	Repeat all words in any order	*Free*
II	4	7–13	4–7	Repeat the word you heard only once	*Selective**
III	8	7–13	4–7	Repeat all words but leave last the one you heard only once	*Restrictive**
IV	4	7–13	4–7	Repeat the word you heard only once	*Selective**
V	7	4–13	4–13	Repeat all words in any order	*Free*

* *Restrictive* and *selective* conditions were interchanged in the schedule with alternate subjects.

The instruction for recall in Sections II and IV and in Section III alternated with subjects within any one age range, calling for partial or complete recall. The method of partial recall, adopted from Yntema and Trask (1963), required the reproduction of the one unrepeated word, and will be referred to as *selective recall*. Complete recall of the same lists was subject to one constraint: the subject was allowed to reproduce the words he remembered in any order but had to leave last the unrepeated word. This method will be referred to as *restrictive recall*. To assure that the instructions were understood, Sections II and III started with two trials that were not scored.

The design controlled for any differences in the ease with which words could be memorized between *selective* and *restrictive* recall. In order to provide comparisons with *free* recall, six subjects in each age group were presented with a tape in which the original lists of four, five, six and seven different words were interchanged between Sections I and V and Section III.

Performance was scored for the number of words correctly reproduced in the test of *free* recall and for correct reproduction of the unrepeated word in *selective* recall. *Restrictive* recall was scored on both these counts, and also for complete recall with the odd word at the end. Every subject who could perform the preceding tasks of the battery was testable by this procedure.

RESULTS

Mean retention span estimated by the number of words reproduced under the conditions of *free* and *restrictive* recall is given in Table II. List size refers to the number of different words, while the total number of items will be indicated by string length. These two measures are interchangeable in *free* but not in *selective* or *restrictive* recall.

TABLE II

MEAN NUMBER OF WORDS REPRODUCED ACCORDING TO AGE

List size*	*Free recall*								*Restrictive recall*				
	4	5	6	7	9	11	13	4–7	4	5	6	7	4–7
Age													
20–29	3·9	4·6	5·2	4·8	5·6	5·6	5·2	4·6	3·9	4·8	4·8	4·9	4·6
30–39	3·9	4·6	5·2	4·7	5·4	5·4	5·0	4·6	3·8	4·6	4·7	4·9	4·5
40–49	4·0	4·6	5·2	4·5	5·2	5·2	4·5	4·6	3·7	4·5	4·6	4·8	4·4
50–59	3·9	4·6	5·4	4·8	5·3	5·0	4·9	4·7	3·6	4·4	4·5	4·6	4·3
60–69	3·8	4·6	5·0	4·5	5·0	5·1	4·7	4·5	3·5	4·4	4·4	4·4	4·2

* List size stands for the number of different words in a string.

Lists of nine or 11 words represent the optimal range for *free* recall measured by items reproduced. The general trend was a drop in recall with age but with none of the string lengths was this a continuous function. Only with lists of nine words or more does this trend appear to be irreversible, and the longer the string the younger the age at which the loss in capacity becomes evident. With strings of nine words the oldest age group differed significantly from the others; with strings of 11 the three younger age groups recalled significantly more words than the two oldest; with strings of 13 the dividing line occurred about the age of 40 years.

Under the conditions of *restrictive* recall the number of words reproduced dropped stepwise from each age group to the next oldest, within the entire range of list sizes tested. Scores composed of the number of words recalled by a subject on all eight trials were subjected to analysis of variance, and showed a significant linear trend as well as between-group difference.

Estimates of the retention span that give credit for fractional recall are not widely accepted, nor do they allow for comparison with estimates based on partial recall. Judged by complete success, the retention span tends to drop with increasing list size, and in the present study did so at all age levels as shown in Table III. The advantage of partial over complete recall became apparent only with lists of six or seven different words, i.e. as success in complete enumeration dropped below the 50 per cent. level.

TABLE III

Successful Recall According to List Size Based on Two Trials per Subject (N = 400)

Recall instruction	*Free*	*Selective*	*Restrictive*		
Method of scoring	*Complete*	*Partial*	*Perfect*†	*Complete*†	*Partial*†
List size*					
4	347	351	294	339	340
5	279	283	245	254	272
6	147	170	48	78	180
7	26	87	11	12	90

* List size stands for the number of different words in a string.

† Under *restrictive* instruction, perfect recall means faultless performance; complete recall stands for the reproduction of the entire list without regard to order; partial recall refers to the correct terminal placement of the unrepeated word.

Any progressive effect ageing exerts on the immediate memory span can best be observed when performance scores are pooled over the eight trials, either by crediting a subject with his successes irrespective of list size, or by weighting his score in proportion to the words recalled. This was done for *free* and for *selective* recall on the basis of full compliance with the instruction. For *restrictive* recall two additional criteria were adopted in order to provide alternate data for comparison with *free* and *selective* recall. Perfect recall under *restrictive* conditions stands for the enumeration of the entire list with the unrepeated word left to the end; complete recall stands for the enumeration of all the words in a list in any order; partial recall stands for the reproduction of the unrepeated word in a terminal position, though not necessarily following the enumeration of all other items. Since no list longer than seven words was recalled in full, individual scores in *free*, as well as in *selective* and—under the three criteria of—*restrictive* recall ranged from zero to eight. As these scores of successful trials made no distinction according to task difficulty, i.e. the size of the string recalled, a second set of individual scores was calculated by weighting each

TABLE IV

Mean Success of Recall in Eight Trials and Recall Score Weighted by List Size* According to Age

	Mean Success					*Weighted Recall Score*				
Instruction	*Free*	*Selective*	*Restrictive*			*Free*	*Selective*	*Restrictive*		
Score	*Complete*	*Partial*	*Perfect*†	*Complete*†	*Partial*†	*Complete*	*Partial*	*Perfect*†	*Complete*†	*Partial*†
Age										
20–29	3·98	4·58	3·62	3·90	5·15	19·20	22·92	16·90	18·35	26·45
30–39	4·10	4·40	3·38	3·55	4·88	19·50	22·42	15·70	16·72	24·58
40–49	3·88	4·30	3·00	3·42	4·42	18·12	21·82	13·32	15·45	22·20
50–59	4·28	4·30	2·68	3·12	4·18	20·65	21·58	12·98	14·60	21·32
60–69	3·98	4·32	2·38	2·78	3·80	19·02	21·58	10·92	12·70	18·80

* List size stands for the number of different words in a string.

† Under *restrictive* instruction, perfect recall means faultless performance; complete recall stands for the reproduction of the entire list without regard to order; partial recall refers to the correct terminal placement of the unrepeated word.

success by the number of different words in the list. These scores ranged between zero and 44, and group means are listed in Table IV alongside means of successful recall without regard to list size.

Table IV shows group means of the two sets of performance scores according to instruction for recall and criterion of compliance. It is apparent that no systematic ageing effect emerged in *free* recall. Performance in *selective* recall tended to deteriorate with successive decades, but this trend did not prove to be statistically reliable. Analysis of variance calculated on mean success or on weighted scores showed no significant between-(age) group variances or linear trends, either for *free* or for *selective* recall. Analysis of variance calculated on each of the six arrays of scores under *restrictive* recall indicated between-group variances and linear trends significantly ($p < 0{\cdot}01$) larger than within-group variance.

TABLE V

ERRORS BY INCORRECT RESPONSE ACCORDING TO AGE (N = 80) AND LIST SIZE* (N = 100)

	Free	*Selective*	*Restrictive: Complete*	*Restrictive: Partial*
Age				
20–29	29	24	18	17
30–39	18	22	15	19
40–49	22	18	15	21
50–59	21	24	18	28
60–69	29	21	19	33
List size*				
4	12	10	16	13
5	19	22	15	21
6	28	40	23	38
7	60	37	31	46
Total	119	110	85	118

* List size stands for the number of different words in a string.

Most subjects correctly recalled both strings of four words under all instructions, but few succeeded twice with strings of seven. Repeated success occurred more frequently with partial than with complete recall, and was inversely related to age under *restrictive* instruction. Neither in *selective* nor in *restrictive* recall did the distance of the unrepeated word from the end of the list exert a significant influence on its recall, or on the number of other words recalled. Errors by false positive response, as listed in Table V, also show a progressive trend with age under *restrictive* instruction. These errors were almost exclusively instances of intra-list intrusion.

DISCUSSION

While there is little doubt that the capacity to acquire new information declines with the advancing years, the immediate word span reflects that deficit no more than does the digit span between the ages of 20 and 70 years. Lists of four, five, six or seven words were recalled correctly about as often by the oldest as by the youngest or intermediate age groups. When complete recall failed, and performance was judged for the number of words retained, there appeared an inverse relation between the word span and age, but this trend was neither progressive nor stable. The experiment furnishes no evidence for the view that the interference effect of recall with short term storage increases over the first five decades of adulthood.

Free recall demands the least organization of the input for registration. Some forgetting, nevertheless, occurred under that condition at all age levels. It should be attributed to interference by the operations of recall and, past the immediate retention span, also to interference by the message units in excess of that limit. Failure in *selective* recall stemmed from the operations of search and matching, and from such sub-vocal recital as may be necessary to carry out those operations. Previous experiments (Anderson, 1960; Sperling, 1960; Buschke, 1963) have shown that these operations result in less interference than does vocal enumeration without the requirement of selection. The present experiment confirmed their observation only with lists that exceeded the length of the immediate retention span. Comparisons based on string length (the number of items presented) would, of course, support the argument that partial recall yields more generous estimates of the immediate memory span than does complete recall. Such comparisons, however, would be misleading, for it is quite apparent from matching successes in *free* and *restrictive* recall that the only acceptable measure of task difficulty was list size, i.e. the number of different words in a string.

Interference effects in *free* recall did not change with age; those in *selective* recall increased somewhat with the years but not significantly. Changes in the word span that could be unambiguously attributed to ageing came into view only with *restrictive* recall. Rather unexpectedly, in the younger groups the combined effect of interference by overt recall and by sorting, matching and holding one selected item, did not exceed that of either source alone. The gain derived from repeated presentation may have contributed to the parity of *free* and *restrictive* recall. Repetition was a condition common to *selective* and *restrictive* recall, and probably helped in listing the duplicated words but, whether that attempt was successful or not, it certainly did not help in holding to the end the unrepeated word. Apparently, the sorting and matching demanded by *selective* recall presupposes the enumeration of the other items—albeit subvocally.

With increasing age the influences of the helpful and taxing experimental conditions—repetition versus selection and holding—ceased to be balanced. Whatever younger men may gain from the second presentation of a message is insufficient in the more advanced years to offset the immediate forgetting that occurs when part of the message is held in storage while the remainder is reproduced. On the evidence of the *free* recall scores interference by enumeration does not account for this ageing effect; on the evidence of *selective* recall it does not arise merely from retrospective scanning, sorting and matching. Only when selection is combined with vocal enumeration and a storage process is superimposed on these does the ageing effect emerge. It seems more like increased interference by than with retention.

This finding fits comfortably into Broadbent's (1958) model of perception and immediate memory, with its limited capacity system that serves both the operations of selection and enumeration, and also those of storage by means of rapid recirculation. Age slows down the rate at which this system functions, more especially at which it recirculates messages and thus allows for an effective division of its functions. Welford (1958) reported deterioration with age in problem-solving, as well as in learning and perceptual-motor tasks that demand the simultaneous gathering and holding in mind of data, or multiple transpositions. The present author observed a similar ageing effect in tests of the running digit span that required transposition of the auditory information into written records.

Does then a decline in immediate memory capacity account for an increasing tendency to forget with age? Messages of four or five unrelated items to be retained in full, or scrutinized for one item to be retained, are hardly typical of memory tasks in everyday situations. The requirement to perform operations of selection, ordering—into pre-existing filing categories as well as within the incoming message—and matching against models, while subvocally rehearsing the input, is quite common. Moreover, such cognitive operations are also necessary in order to extend the immediate memory span beyond the three or four items that may indeed represent the range of a sensory capacity which does not deteriorate with age.

In conclusion, it should be noted that in this experiment the immediate word span was much shorter than that reported by other investigators, typically working with college students. In the present study even the youngest men attained a lower level of success, and also reported a smaller proportion of the words from lists they failed to recall in full. Differences in motivation and practice, as well as in intellectual capacity, may account for these discrepencies. Such differences as appeared with age, however, clearly represent changes in capacity, for no general construct of motivation could explain the absence of ageing effects under one experimental instruction and their emergence under another.

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EXPOSURE DURATION AND EFFECTIVE FIGURE-GROUND CONTRAST

BY

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The pre- and post-exposure fields in the tachistoscopic presentation are assumed to reduce the apparent contrast of the figure by brightness summation. A matching procedure was used to measure this effect. Apparent contrast rises linearly with duration, but only in the upper range. Further observations confirm the suggestion that the pre- and post-exposure fields retard the formation of bounding contours with a further reduction of apparent contrast at short durations as a result. It is indicated that the contrast-matching method provides a short-cut technique for the measurement of the temporal range of brightness summation.

The tachistoscope is by far the most popular of the instruments used by psychologists in their efforts to make accurate perception difficult or impossible: any patterned stimulus can be made subliminal if it is presented for a sufficiently brief interval. In what may be termed standard tachistoscopic usage, two or more fields are quickly alternated: an adapting field (usually including a fixation mark), and a target field; the post-exposure field may or may not be identical to the adapting field. In most cases, the various fields are equated for luminance. Surprisingly, there has been relatively little interest in the questions of why a figure briefly presented under such conditions is subliminal.

One assumption seems to be that variations of duration are equivalent in their effects to corresponding variations of luminance according to the familiar reciprocity relation (Graham and Ratoosh, 1962). However, this relation has never been systematically studied under tachistoscopic conditions, as defined here. Another possible assumption is that the post-exposure field which immediately follows the target prevents after-images and persistence effects (Dodge, 1907), so that the duration of the psychological "image" of the target corresponds to the duration of exposure. In recent studies which relied on this interpretation, the tachistoscopic situation has sometimes been modified by presenting visual noise in the post-exposure field (Averbach, 1963; Sperling, 1963).

It is also possible to interpret standard tachistoscopic conditions in terms of visual masking by light. Recent investigations of the forward and backward masking effects of light on the perception of black-on-white targets (Eriksen and Hoffman, 1963; Kahneman, 1965) have suggested that such effects can be explained, at least in part, in terms of brightness summation. The areas which are black in the target figure are illuminated in the adapting and masking fields; if brightness summation occurs over the "black" area, it should appear greyish rather than black. The pre- and post-exposure illumination of the field thus decreases the contrast between the figure and its background. A figure which is briefly presented under tachistoscopic conditions may fail to be perceived if it does not present sufficient contrast against its background. Thus, the duration threshold represents at least in part a contrast threshold.

In elaborating this model, it is important to distinguish between three concepts of figure-ground contrast:

The *physical* contrast between figure and ground is defined by formula (1), following Luckiesh (1944, p. 108):

$$C = \left(1 - \frac{I_F}{I}\right)100 \quad \cdots \quad \cdots \quad \cdots \quad \cdots \quad (1)$$

where I_F is the luminance of the darker figure area, and I is the luminance of the background. Physical contrast is thus defined quite independently of any properties of the observer.

The *effective* contrast between figure and ground is a hypothetical characteristic of the physiological response to a patterned stimulus. As a first approximation, we assume that effective contrast is determined by the ratio of the luminous flux which reaches the observer's eye from the figure to the amount of flux from the background, during a period T of perfect brightness summation. In tachistoscopic conditions, a figure of physical contrast C is briefly presented on a background of luminance I, being preceded and followed by a homogeneous field of the same luminance. According to the present hypothesis, effective contrast should take the following value for brief durations of exposure ($t \leqslant T$):

$$C' = \left(\frac{1 - I_F{\cdot}t + I{\cdot}(T - t)}{I{\cdot}T}\right)100 \quad \cdots \quad \cdots \quad \cdots \quad (2)$$

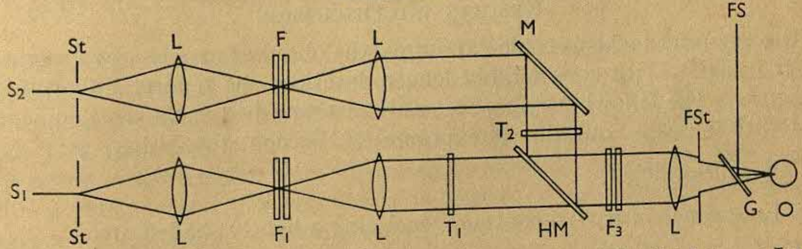
It follows from (2) that the effective figure-ground contrast of a target exposed for brief durations should be a linear function of the exposure duration t, which passes through the origin and reaches the value C at the duration $t = T$.

The *apparent* contrast between a figure and its background is its subjective location on the black-white continuum. Apparent contrast is one possible measure of effective contrast. It is also possible to use acuity performance as a less direct measure of effective contrast, in view of the known effects of figure-ground contrast on such performance (Luckiesh, 1944; Weymouth, 1955). In the present study, an attempt is made to test some implications of formula (2), with apparent contrast as the measure of effective contrast.

METHOD

Apparatus. A schematic diagram of the optical set-up is presented in Figure 1. The subject's head is immobilized by a bite-board. He sees in Maxwellian view a circular illuminated field subtending 8°. Illumination is provided by two light sources S_1 and S_2

FIGURE 1



Optical arrangement (not to scale). S_1, S_2: light sources; St, FSt: field-stops; L: lenses; F_1, F, F_3: filters; T_1, T_2: targets on transparencies; M: mirror; HM: half-silvered mirror; FS: fixation source; G: glass; O: observer.

whose intensity may be separately controlled by neutral density filters at F_1 and F. Additional filters may be added in the common path at F_3. A square piece of opaque material is located at T_1. When only S_1 is turned on, the observer (O) sees a sharply outlined black square. The source S_2 provides homogeneous illumination over the entire field. When the two lights are turned on simultaneously, this arrangement can provide any required degree of figure-ground contrast of the dark square by appropriate adjustment of the illumination in the two paths. For example, when S_1 and S_2 provide equal illumination at the eye, physical contrast is 50 per cent.

The light sources are Sylvania R1131C glow modulator tubes. Temporal parameters are programmed and controlled by an Iconix FAST System.

Procedure. The psychophysical method used in this study was originally intended only for pilot investigations, because of its many obvious drawbacks. However, it soon appeared that this method produces adequately reliable and consistent results in a very economical manner, and it was therefore retained for the more systematic observations reported here.

The procedure requires the observer to match two exposures of a figure for apparent contrast.

The standard stimulus consists of a 2 sec. exposure of a square subtending 4°24′, at a particular value of physical contrast. The standard stimulus is presented as many times as required by the observer, with dark intervals of 1·5 sec. between successive presentations.

The comparison stimulus consists of the following sequences:

(1) Homogeneous field—1 sec.

(2) Presentation of square at 100 per cent. physical contrast—variable duration (*t*).

(3) Homogeneous field—1 sec.

The observer's task is to find a duration *t* at which the comparison stimulus is of the same apparent contrast as the standard stimulus. The first comparison stimulus is chosen by the experimenter, who selects a duration between 1 millisec. and 50 millisec. from a table of random numbers. The observer then requests that this duration be changed, specifying both direction and magnitude of change (in millisec.). The procedure is repeated until he is satisfied that the required match has been obtained. By such manipulations of duration, the observer adjusts the apparent contrast of the comparison stimulus to a match with the standard. He is informed neither of the physical contrast of the standard stimulus nor of the starting point of the comparison series.

The entire procedure for one measurement usually requires less than 2 min. About 10 sec. are required to adjust the setting from the standard to the comparison conditions. Changes of the duration *t* take less than 2 sec.

Measurements were obtained for four levels of background luminance, ranging from 40 ml. to 0·0013 ml. in steps of 1·5 log units. Two experienced observers participated in the experiment. For each of them, two series of measurements were obtained at each luminance level, for values of physical contrast in the standard exposure ranging from 10 to 80 per cent. in steps of 10 per cent. For one observer, a single session consisted of two series of measurements at the same luminance level. For the other, two different levels of background luminance were investigated in each session. This difference in treatment was intended to provide information concerning the stability of results.

Results and Discussion

With very brief tachistoscopic exposures, the observer merely sees a vague flicker across the field. With considerably longer durations, he reports a sharply defined black figure. The following stages are easily distinguished in the development of the percept with increased duration of exposure (cf. Vernon, 1952, Chap. 3):

(1) Formless flicker.

(2) Emergence of partial contours, enclosing a lightly shaded area.

(3) Completion of contours, increase in shaded area, darkening of shading.

(4) "Filling-in" of figure, homogeneous shading over the entire area.

Three processes are involved in this description: (*a*) contour formation; (*b*) darkening of shaded area within contours; (*c*) filling-in of shaded area. The present matching procedure is intended to measure the development of process (*b*) with increasing exposure duration. Thus, the greyish, washed-out appearance of the target is the basis on which the comparison figure is matched to a given value of physical contrast in the standard stimulus. The observer is required to abstract this feature from the many aspects in which the two figures differ in appearance. At the lowest durations, it appears impossible to judge the darkness of the shadow which is all that is seen in stage 1. At the higher durations, it becomes very difficult to judge the apparent darkness of the figure independently from the degree to which it is "filled-in." For this reason, no readings for contrast-values higher than 80 per cent. were taken in the present study. Even when the shaded areas appear quite dark (e.g. for the durations which are matched to 80 per cent. physical contrast) the process of figure formation is far from complete and the standard and comparison stimuli differ greatly in appearance.

TABLE I

Durations of Exposure of a Black Square under Tachistoscopic Conditions which Match in Apparent Contrast Eight given Values of Physical Contrast, at Four Levels of Background Luminance, for Two Observers

Back-ground luminance	40 *ml.*		1·3 *ml.*		0·04 *ml.*		0·0013 *ml.*	
Subject	*J. N.*	*D. K.*	*J. N.*	*D. K.*	*J. N.*	*D. K.*	*J. N.*	*D. K.*
Contrast per cent.								
10	4·5	2·7	5·0	4·0	10·0	14·2	40·5	34·0
20	6·5	4·0	6·5	6·0	13·0	16·0	39·0	41·0
30	7·0	5·5	9·5	6·5	15·5	21·0	51·5	49·5
40	7·5	7·5	9·5	9·5	17·0	24·0	51·0	56·5
50	10·0	6·5	12·5	8·5	21·5	23·5	60·0	62·0
60	11·0	10·5	14·0	13·0	27·5	28·0	61·0	68·0
70	13·0	11·0	17·0	13·0	30·5	29·0	69·0	68·0
80	17·5	12·0	21·0	15·5	38·5	38·0	83·5	80·5

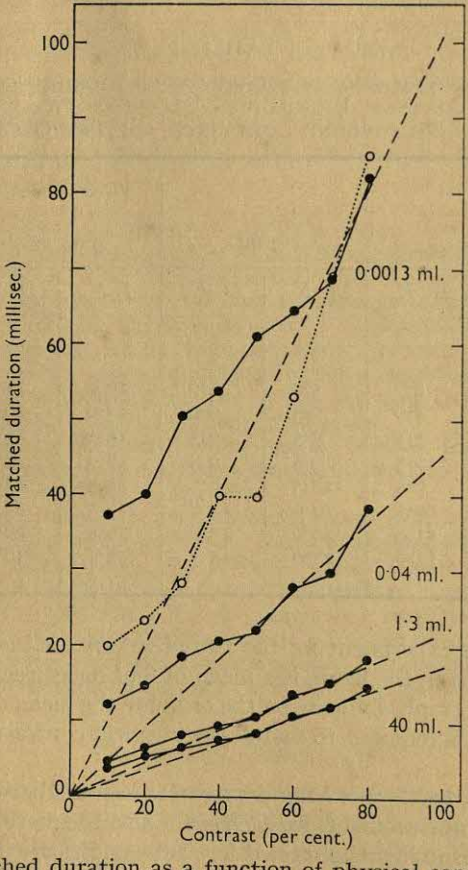
The results of the experiment for the two observers are presented in Table I and Figure 2. Each point in the figure is a mean of four measurements, two each for the two observers. The graphs indicate on the ordinate the mean duration of exposure of a black figure which is required to match the phsyical contrast levels marked on the abscissa.

As the table indicates there is a fair degree of agreement between the two observers. The repeatability of measurements for each one is also adequate. The mean difference between the two measurements obtained for each condition is the same (3·8 millisec.) for both J. N. (who took both measurements in the same session), and D. K. (whose corresponding measurements were separated by at least one day). This amount of variation is slight when compared to the magnitude of the experimental effect.

However, a systematic inconsistency was observed for the highest level of background luminance, which was the first to be investigated in preliminary experiments: the exposure required to match any level of contrast tended to become systematically shorter from session to session, although the results within any one session were quite consistent. Both observers returned to this condition at the close of the experiment, and those late measurements are reported here. Subsequent checks indicated that measurements had stabilized at that time. It is not known whether these changes reflect genuine perceptual learning (the same exposure becoming "blacker" as a result of experience) or whether they are due to judgement effects, but the phenomenology of the experiment suggests that the former interpretation may be more nearly correct.

It is evident in the data of Figure 2 that the apparent contrast of a briefly exposed black-on-white target increases consistently with exposure duration. However, one of the predictions derived from formula (2) is clearly not verified: the function relating exposure duration to contrast does not pass through the origin. There appears to be a threshold value below which figure-ground contrast is absent. Beyond this threshold value, apparent contrast rises slowly at first, then more rapidly with increasing duration. For the three highest levels of background luminance, the function appears to change its slope around 50 per cent. contrast; the data points corresponding to contrast levels of 50–80 per cent. seem to fall on lines passing through the origin. A similar line was also drawn for the lowest background luminance (top graph), although the fit is mediocre in this case.

FIGURE 2



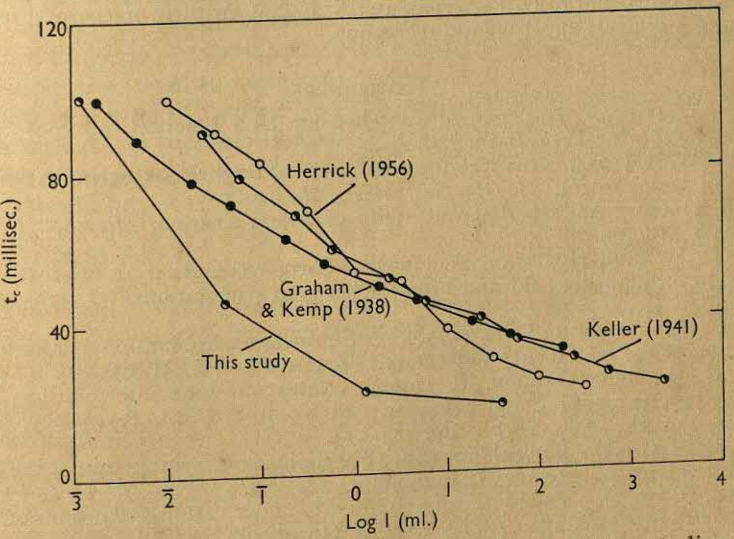
Filled circles: matched duration as a function of physical contrast, at four levels of background luminance. Mean for two observers.

Unfilled circles: target preceded and followed by thin outline. See text.

The following interpretation of these findings may be advanced: the apparent contrast of a figure is only partly determined by brightness summation. Apparent contrast also depends on the formation of some bounding contours, which are absent at short durations of exposure. According to this interpretation, the duration which is obtained by extrapolating the lower segment of each function to zero-contrast represents a lower threshold for the formation of bounding contours in the tachistoscopic situation. The values of this contour threshold are certainly determined by masking effects of the pre- and post-exposure fields: fully formed figures are easily visible when these fields are omitted, at durations so short that only flicker is seen in the usual tachistoscopic situation. The adequate fit to the summation model obtained for longer durations of exposure suggests that contour effects cease to be limiting at these durations.

According to this interpretation, the pre- and post-exposure fields reduce apparent contrast in two ways: directly, by brightness summation; indirectly, by retarding the formation of contours which are essential to the appearance of contrast. The validity of this interpretation was tested in a further set of observations. The procedure was as in the main experiment, with one modification: a thin outline of the square (stroke width, 5′) was drawn in India ink on a transparency and was presented at T_2 (see Fig. 1). This outline appeared in the pre- and post-exposure fields. Thus, the contours of the square are "pre-formed" in this condition, and a closer fit to the brightness-summation model is therefore anticipated. The results are presented as unfilled circles in Figure 2. Measurements were conducted only at the lowest level of luminance, for which deviations from linearity were most pronounced in the original data. As predicted, the results in the two conditions are similar at the higher values of contrast, but apparent contrast is much higher at short durations of exposure when contours are presented in the pre- and post-exposure fields.

FIGURE 3



Critical duration (t_c) as a function of log adapting luminance in three studies, compared to T estimated by extrapolation of the broken lines in Figure 2 of this study.

The present discussion has suggested that the durations attained by extrapolating the broken lines through the origin to 100 per cent. contrast represent upper limits of brightness summation, indicated by T in formula (2). A more customary approach to the study of brightness summation utilizes the reciprocity design, in which the critical duration is defined as the value beyond which the energy (luminance × duration) required for some threshold performance begins to rise. In Figure 3, the values of *T* obtained in the present study are compared with critical durations obtained in several studies of the reciprocity law in brightness discrimination. There are important differences in the experimental conditions of these studies. In the experiments of Graham and Kemp (1938) and Keller (1941), a luminance increment is applied to one half of a constantly exposed illuminated circle subtending approximately 1·5°. In Herrick's study (1956), the subject must detect an increment or decrement of luminance applied to the whole of a circular field subtending 1°. It can be seen that the values of *T* obtained in the present experiment are markedly lower than critical durations reported in these studies, at comparable levels of background luminance. The curve summarizing the results of the present study appears displaced approximately one log unit to the left respective to the other curves in the figure. The general range of summation and the decrease of summation with increasing luminance of background are quite similar. Further studies of brightness summation comparing the two methods are required to establish the causes of the discrepancies observed in Figure 3.

It may be of some methodological importance to establish with certainty the correspondence between estimates of *T* by contrast-matching and by the more elaborate measurements of critical durations. An adequately reliable measure of the range of summation may be obtained by matching three or four values of contrast and extrapolating a line through the origin to 100 per cent. contrast, a procedure which requires a total of 12–15 min. of experimental work. A short-cut technique of this kind may be valuable in the study of fluctuations of brightness summation as a function of physiological and psychological variables.

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AN ADJACENCY EFFECT IN FREE RECALL

BY

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An *adjacency effect* was demonstrated at a high level of significance in the free recall, by 123 subjects, of a list of 40 high-frequency nouns presented in varying order on successive trials. The phenomenon referred to as the adjacency effect consists of the fact that when a subject is given repeated trials of study and free recall of a list of words (always presented in a different order), the probability of recalling a given item is greater when the item is presented temporally adjacent to an item which is already learned (as evidenced by recall on the previous trial) than when the item stands temporally between other items which are not yet learned. The enhancement of recall is greater when the item is presented between two previously learned items. The implications of the adjacency effect for verbal learning theory, particularly for the serial-position effect in serial learning and the concepts of interference and neural consolidation, are discussed.

INTRODUCTION

The experiment reported here is directly relevant to two familiar topics in the psychology of verbal learning: (*a*) serial learning, particularly the determinants of the serial-position effect, and (*b*) free recall, particularly the determinants of the recall of individual items when there are repeated presentations of the list.

The rationale of the present experiment is based on an hypothesis originally suggested by the writer to help explain the well-known serial-position effect found in serial rote learning by the anticipation method (Jensen, 1962). It was noted that subjects learn the items of a serial list in a rather definite order, as if the first item in the list (or possibly the signal to anticipate the first item, or even the blank inter-trial interval) acts as a kind of "anchor" point around which the other items of the list become "attached" on successive trials. For example, the rank order in which the items of, say, a nine-item list are typically learned (i.e. attain some designated criterion of mastery) would be:

Serial position:	1	2	3	4	5	6	7	8	9
Order of learning:	1	2	4	6	8	9	7	5	3

If the item in Position 1 is regarded as the anchor point, the following rearrangement of the list illustrates more clearly how the items appear to become "attached" around the anchor on successive trials (assume one item learned per trial):

Serial position:	6	7	8	9	1	2	3	4	5
Order of learning:	9	7	5	3	1	2	4	6	8

The distinctive skewed bow-shape of the serial-position curve can be thought of as being a direct consequence of this order of learning the items of a serial list. The descriptive evidence concerning this proposition has been presented in detail elsewhere (Jensen, 1962; Jensen, 1963; Jensen and Roden, 1963).

The crucial question, of course, is what causes this particular order of learning. A possible explanation was put forward by Jensen (1962), which is referred to as the *adjacency* hypothesis. This states that an item is more readily learned if it is presented adjacent to another already-learned item than if it is presented adjacent to other items that are as yet unlearned. We are concerned here with adjacency in temporal, rather than spatial, order. The adjacency effect might also be manifested in spatial arrangements and would be sought in the simultaneous presentation of the items of a serial list over a number of trials. The adjacency hypothesis now being considered, however, concerns adjacency in the temporal sequence of presentation of stimuli, as in the usual case of serial rote learning by the method of anticipation. According to the adjacency hypothesis, if the subject learns only the first item of a serial list on the first trial, the item he will most probably acquire on the next trial will be the item in Position 2, since it is temporally adjacent to the item in Position 1. The next one to be learned will be the item in the last position, since it is the next most temporally adjacent to Position 1; then the next one to be learned will be Position 3, because of its adjacency to the already-learned Position 2; and so on, in the order which has been described above and which has already been substantiated for a wide variety of serial learning conditions (Jensen, 1962).

If the adjacency hypothesis is to be used in explaining the serial-position effect, however, the adjacency principle must itself be a more general and fundamental effect than the phenomenon it is intended to explain. Adjacency must therefore be regarded as a more basic principle of learning than the serial position phenomenon, and it must be capable of being shown to operate in other forms of learning than serial learning. Preferably it should be demonstrated in some simpler type of learning than serial learning.

In one sense, at least, free recall can be considered a simpler form of learning than serial learning, in that the subject is required only to reproduce the items in the list in free recall, without having to recall them in any prescribed order. The question therefore arises of whether the hypothesized adjacency effect can be found in free recall under the appropriate conditions for its demonstration, viz. a list which is (*a*) composed of too many items to allow complete recall after a single presentation and (*b*) which gives the subject the opportunity for repeated trials presented in a manner of study and recall. In other words, we are asking whether the adjacency effect can be demonstrated under conditions which are quite different from those of serial rote learning, i.e. where there is no constant order of presentation of items and where subjects are instructed only to recall as many of the items as they possibly can, without regard to their order of presentation.

The literature on free recall was searched for some indication of the adjacency effect, but no hint of it was found. The variables that have been found to determine the probability of recall of any given item in a free recall test, however, are numerous: the length of the total list in which the item occurs; the serial position of the item in the list; the rate of presentation of items; the frequency of the item in the language; the item's association value; the rated meaningfulness of the item; the "interest" value of the item to the particular subject; the strength of previously established association between items in the list (inter item associative strength); the degree of intra-list similarity, and conversely, the degree of "isolation" of the item; the degree to which the sequence of items approximates the sequential probabilities of normal language; the degree of opportunity for clustering; and the number of classificatory categories provided by the list. A number of these variables, of course, overlap one another in the total variance accounted for. But if they were all combined into a multiple regression equation probably a quite good job could be done of predicting the probability of recall of individual items under various conditions.

If the hypothesized adjacency effect were substantiated, it would be an important addition to this list and to our understanding of the psychological processes underlying free recall. Adjacency might even be regarded as a more fundamental determinant in free recall than many of the other variables listed above, in the sense that it may be less dependent upon antecedent experiences and may be a more intrinsic property of the learning process. Therefore, the adjacency hypothesis seemed worth testing: if the adjacency effect were demonstrated we would have one more principle of free recall and we would also be provided with some knowledge of a process which might contribute to the formulation of a more satisfactory theory of serial learning and the serial-position effect.

METHOD

Materials

In attempting to detect the adjacency effect in a free recall situation it seemed advisable to try and minimize the role of other variables known to affect free recall. So the lists were made as homogeneous as possible with respect to such potent variables as familiarity and meaningfulness. In fact, the attempt was made to have lists in which all the words might be regarded as being asymptotic with respect to familiarity.

Thus, a pool of 100 words was made up having the following characteristics: (*a*) all concrete, common nouns, (*b*) no fewer than three nor more than six letters, (*c*) high familiarity as indicated by membership in the AA category (at least 100 per million) of the Thorndike-Lorge (1944) frequency count, (*d*) as many different initial letters as possible (*q*, *x* and *z* are the only ones not represented), and (*e*) easy spelling as indicated by correct spelling by at least 90 per cent. of eighth graders, according to the Iowa Spelling Scale (Green, 1954).

To enhance the generality of the findings, four "equivalent" forms of a free recall test were composed from this pool of 100 words. Each of the forms involved 60 words, so the forms were partially overlapping. All 100 words, however, occurred with equal frequency within the total of all four forms.

The same procedure was used for composing each of the forms. In one form there were six lists, with 40 words in each list. Lists 1, 2, and 3 consisted of the same 40 words, but they were always presented in a different order. List 4 consisted of only 20 words from the three previous lists (Lists 1, 2, and 3) and 20 "new" words. All the words of List 4 were repeated in Lists 5 and 6, always in a different order. Thus, subjects were presented with the same list of 40 words, each time in a different order, for three trials; then 20 of these words were discarded and 20 "new" ones were added, and this list was presented for three more trials, again with a different order of the words on each trial.

The problem of the serial-position effect was handled in a systematic fashion, the aim being to minimize the effect as much as possible. In hopes of accomplishing this the following restrictions were imposed on the ordering of the words on each successive list.

Think of the list of 40 words as being divided up into eight equal sections of five words each, i.e. Sections I to VIII. Then consider any two successive lists, say, Lists 1 and 2. The rules of ordering were: (*a*) A word should not occur again in the same section of the list or in either of the two adjacent sections. This insures that each word holds a variety of serial positions on successive trials. (*b*) Words in Sections I and VIII should not appear in either of these sections or in Sections II or VII on succeeding trials. This insures that no word will benefit from "primacy" or "recency" on more than one trial. (*c*) No more than one of the five words within a given section on one list should appear in any one section on the succeeding list. This insures that the words will not maintain any temporal proximity to one another from trial to trial. (*d*) Two words should never be adjacent to one another more than once in all six lists. (*e*) Any pair of adjacent words which were judged to suggest some rather common pre-experimental association which might cause them to be recalled together was broken up and rearranged (always in accord with the other rules). For example, the adjacent words *fire* and *bird* had to be broken up to avoid the suggestion of Stravinsky's *Firebird*.

The 20 words discarded from the list prior to Trial 4 were eliminated on a random basis. The 20 "new" words that were added (from the pool of 100 words) were unsystematically intermixed with the "old" words. The ordering of the old words, however, was strictly in accord with the rules outlined above.

Procedure

Subjects were tested in groups of approximately 30. To guard against copying, they were required to leave at least one seat vacant between them and to put all books, etc., under their seats. Each of the four groups received a different form of the test, as described in the preceding section. Aside from the different forms, the procedure was the same for all groups.

Subjects were instructed to attend to the series of words presented on the screen, and, when the room lights were turned on after the presentation of the last word, to write down as many words as they could recall in whatever order the words came to mind. Subjects were urged to record only the words in the list presented just prior to the recall period. Every subject was provided with six sheets of ruled paper, each labelled with the trial number. At the end of each trial subjects placed their answer sheets in a large manilla envelope to insure that the recalled words would be out of sight on subsequent trials.

The words were automatically projected on the screen at the rate of one word every 2 sec., with a 0·5 sec. blank interval between words. The words appeared in black against a white background; the letters (all lower case) stood approximately 5 in. high on the screen. The room was darkened during the presentation. Immediately after the last word in the series, the lights were turned on and subjects were given 4 min. to write the words they could recall and to place the sheets in the manilla envelopes. All six trials proceeded in this fashion.

Subjects

A total of 123 undergraduate students in an introductory course in educational psychology at the University of California served as subjects. They were unsystematically divided into four groups of approximately 30 subjects in each, so that four different sets of word lists could be used.

RESULTS AND DISCUSSION

The data from each of the four groups were first analyzed separately, but since there were no appreciable or significant differences among groups on any of the relevant measures, the data were combined and the results are reported for the composite N of 123.

It should be noted that the free recall performance was quite typical for the type of materials and method of presentation used here. The mean number of words recalled (out of the list of 40) on each of the Trials 1–6 were 15·06, 21·25, 26·75; 21·06, 26·43, and 29·12, respectively. Extralist intrusions accounted for 2·8 per cent. of the total recall; the intrusions were eliminated from the analysis. As one would expect, most of the intrusions appeared to be common associates of words in the list: boy—girl, king—queen, star—sky, fire—water, etc.

Adjacency effect

The following discussion will be simplified if it is constantly kept in mind that the *adjacency effect* always refers to the order of *presentation* of the items and has nothing to do with the order of recall.

Analysis 1a. The optimal condition (Condition A) for adjacency to enhance the probability of recall of item i would exist when item i is presented between two items ($i - I$ and $i + I$) which had been recalled on the previous trial ($t - I$), and which, furthermore, were sufficiently well-learned to be recalled again on trial t. The condition making for the lowest probability of recall of item i (Condition B) would exist when item i is presented between two items ($i - I$ and $i + I$) which had not been recalled on the previous trial ($t - I$), and which, furthermore, had acquired insufficient strength to be recalled on trial t. These two sets of conditions (A vs. B) define the *independent variable* of our first analysis.

The *dependent variable* is the recall (R) or nonrecall (NR) of item i on trial t.

The adjacency effect would be demonstrated by a positive correlation between the independent and dependent variables.

Since the independent and dependent variables are both dichotomous, we can have a 2 × 2 contingency table, i.e. A vs. $B \times R$ vs. NR. An appropriate measure of degree of relationship between the independent and dependent variables in this case is the phi coefficient.* The null hypothesis states that the mean phi over all subjects is zero.

Thus, for each subject, every item of the list (except the first and last, which were omitted from the analyses) from List 2 through List 6 was tabulated in one of the appropriate cells of the 2 × 2 contingency table, and the corrected phi coefficient was computed. (Fortunately, it was possible to do all the tabulations and computations on the IBM 7094 computer!) The phi coefficients thus obtained are treated only as a kind of *score* for each subject. Since the cell entries upon which phi is based are not strictly independent, it would be incorrect to attempt to evaluate the significance of phi by means of the usual chi square test. Nevertheless, the phi thus obtained is a legitimate measure of correlation. But since it is treated merely as a score, it is possible to test the adjacency hypothesis by computing the mean and *SD* of phi over all subjects and use the t test to determine whether the mean phi is significantly greater than zero. Since the hypothesis predicts a positive correlation, a one-tailed t test is called for.

For the contingency described above, the mean phi was 0·216, $SD = 0{\cdot}246$. This value is significantly greater than zero ($t = 9{\cdot}40$, $d.f. = 122$, one-tailed $p < 0{\cdot}0005$). The total range of phi for the entire sample went from $-0{\cdot}24$ to $+0{\cdot}78$. The frequency distribution of phis may be roughly described as follows: $-0{\cdot}24$ to $0 = 21$ per cent., 0 to $+0{\cdot}24 = 30$ per cent., $+0{\cdot}25$ to $+0{\cdot}49 = 40$ per cent., $+0{\cdot}50$ to $+0{\cdot}79 = 19$ per cent.

These results clearly substantiate the adjacency hypothesis. Yet it should be noted that many other factors must obviously play a heavy role in determining the probability of recall of an item, since the adjacency effect, as measured by the present method, accounts for only a small percentage of the total variance and shows a wide spread of values for various subjects. Approximately one-fifth of the subjects in this study did not evince the effect. In these cases adjacency was probably swamped by other determinants of recall. Another possibility is that the adjacency effect might be maximal for a given subject only under a particular rate of presentation, with the effect diminishing in proportion to departure from this rate. Thus it might be possible to demonstrate the adjacency effect in every single subject provided the subjects × presentation rate interaction were taken account of experimentally.

Analysis 1b. When the same type of analysis as above was carried out for each trial separately, the mean phi coefficients for Trials 2 to 6 were 0·11, 0·21, 0·23, 0·24, and 0·26, respectively; all are significant beyond the 0·001 level. The fact that phi increases consistently over trials could mean that the adjacency effect becomes stronger when some of the items in the list are overlearned. It could also mean that other factors, such as serial position, meaningfulness, etc., largely determine recall in the early trials and that adjacency is manifested more clearly in later trials after the other factors have become more or less asymptotic in their influence.

Analysis 2a. The next two analyses were intended to determine the degree of the adjacency effect separately for adjacency of item i to the preceding item $(i - I)$ and to the item following $(i + I)$.

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* Since the magnitude of the phi coefficient is affected by the degree to which the marginal frequencies on either variable depart from a 50–50 split, and since this degree of departure varies from trial to trial and from subject to subject, it seemed advisable to apply the correction suggested by Guilford (1956, pp. 314–315), which, in effect, puts every phi coefficient on the same baseline and also makes phi equivalent in magnitude to the product-moment coefficient of correlation. All the phi coefficients reported here have been thus corrected.

First, the preceding item, $i - I$. The dichotomous independent variable is: item $i - I$ recalled on the previous trial ($t - I$) and on trial t vs. item $i - I$ not recalled on trial $t - I$ nor on trial t. The dichotomous dependent variable is: item i recalled on trial t vs. item i not recalled on trial t.

In this case the mean phi ceofficient was 0·07, $SD = 0{\cdot}19$ ($t = 4{\cdot}05$, $d.f. = 122$, one-tailed $p < 0{\cdot}0005$). Though the effect is again highly significant, it is actually of quite small magnitude.

Analysis 2b. Here we are concerned with the effect of adjacency of item i to the following item, $i + I$. The independent variable is: item $i + I$ recalled on the previous trial ($t - I$) and on trial t vs. item $i + I$ not recalled on trial $t - I$ nor on trial t. The dependent variable is: item i recalled on trial t vs. item i not recalled on trial t.

In this case the mean phi was 0·09, $SD = 0{\cdot}27$ ($t = 3{\cdot}67$, $d.f. = 122$, one tailed $p < 0{\cdot}0005$).

The "forward" and "backward" adjacency effects are thus of approximately the same magnitude, and the two effects separately do not produce nearly as great an effect as they do in combination, which indicates a strong interaction between "forward" and "backward" adjacency.

Adjacency and the serial position effect

Since the adjacency effect was originally hypothesized to explain the serial position effect in serial learning, the next analysis was intended to determine the precise relevance of the adjacency effect to the serial position effect.

In order to produce, or to contribute to, the serial position effect, adjacency would have to operate under somewhat different conditions from those selected for our previous analyses. Consider the way in which a serial list is typically learned: The order of learning the items begins with the extremes of the list and proceeds towards the middle, with the forward rate of progress being about one item ahead of the progression from the end of the list. To facilitate explanation, the process can be simply represented by the serial list *abcdef*. Items not learned are represented by lower case letters and learned items by capitals. Thus, on Trial 1 (following the study trial) we have *Abcdef*, on Trial 2 we have *ABcdef*, on Trial 3: *ABcdeF*, Trial 4: *ABCdeF*, Trial 5: *ABCdEF*, and Trial 6: *ABCDEF*.

Now consider the situation that exists after Trial 1: *A* has been learned, and on the next presentation of the list *b* will be temporally the most adjacent to *A*. The adjacency hypothesis, therefore, would predict that *b* should be the next item to be learned. But note that *b* is followed by *c*, which also is not yet learned—neither was it recalled on the preceding trial nor will it be recalled on the trial for which we predict the recall of *B*. The crucial question, then, is: how strong is the adjacency effect under this particular condition as it exists in our free recall situation?

Analysis 3a. This analysis is intended to provide the answer. The independent variable in this case is: item $i - I$ recalled on the previous trial ($t - I$) and on trial t, and item $i + I$ not recalled on trial $t - I$ nor on trial t vs. $i - I$ not recalled on trial $t - I$ nor on trial t, and item $i + I$ not recalled on trial $t - I$ nor on trial t. Again, the dependent variable is recall vs. non-recall of item i on trial t.

The mean phi in this case was 0·11, $SD = 0{\cdot}23$ ($t = 5{\cdot}26$, $d.f. = 122$, one-tailed $p < 0{\cdot}0005$). Adjacency is obviously manifested in the forward direction under these special conditions, that is, a learned item facilitates the acquisition of any item that immediately follows it, though the effect is not especially strong, as indicated by a phi of only 0·11. Now, what about adjacency in the backward direction?

Analysis 3b. In the serial learning paradigm above, note the condition that prevails after Trial 4; the next item that should benefit from adjacency, according to our hypothesis, is item E. In this case, the preceding item (d) has not yet been learned, while the following item (F) has been learned. The corresponding independent variable for our analysis is: item $i - 1$ not recalled on trial $t - 1$ nor on trial t and item $i + 1$ recalled on trial $t - 1$ and on trial t vs. item $i - 1$ not recalled on trial $t - 1$ nor on trial t. The dependent variable is recall vs. non-recall of item i on trial t.

The mean phi in this case was 0·01, $SD = 0{\cdot}21$, which does not differ significantly from zero. It is interesting to note that the adjacency effect was not manifested under these conditions, although it showed up very significantly in Analysis 2b, in which there was no regard for the state of the preceding item. Apparently the preceding item ($i - 1$) carries more weight in the interaction between $i - 1$ and $i + 1$. Adjacency to $i - 1$ alone is enough to yield the effect, while adjacency to $i + 1$ alone is not. Item $i + 1$ seems to add its increment to the effect only when $i - 1$ is also lending its effect.

This finding is quite bothersome in terms of our hypothesis concerning the relation of the adjacency effect to the serial position effect, for it implies that the order of learning the items of a serial list should progress consistently from the beginning to the end of the list, rather than from the extremes toward the middle, which is in fact the case. Therefore, the relevance of the adjacency phenomenon, at least as it is manifested in free recall, to the serial position effect seems quite doubtful. Furthermore, the small magnitude of the effect also leaves it wanting as an explantaion of the serial position effect. It seems doubtful if an effect which accounts at most for some 5 per cent. of the variance in probability of recall could alone produce such a strikingly powerful phenomenon as the serial position effect. On the other hand, it is possible that many of the factors that determine probability of recall in the free recall situation might not play as important a role in serial learning and thereby would leave greater scope for the operation of adjacency. Certainly there are great enough differences between free recall and serial learning to caution against a too hasty rejection of our original hypothesis. But at the moment the relevance of the adjacency effect to serial learning does not appear very promising.

Adjacency, consolidation, and interference.

What this study has established with a high degree of confidence, however, is the adjacency effect itself. Adjacency is clearly a fact of the process of free recall.

Is there any accounting for this phenomenon in terms of more basic principles? The most obvious explanation is in terms of the amount of time it takes for a subject to "process," "encode," or "rehearse" a given item to make it available for later recall. The term "consolidation" is used here loosely to designate this process, whatever it might actually constitute. If an item requires x sec. to be consolidated in the subject's memory for later recall, and if the item is presented for something less than x sec. and is followed immediately by another non-consolidated item, one of two things can happen: (*a*) either the consolidation of the first item is interfered with by the appearance of the second item and the consolidation of the second item is begun, or (*b*) the consolidation of the first item continues until it is complete, thereby overlapping the presentation of the second item and interfering with its consolidation. Once an item has been consolidated, it need not use up further time for this process. Therefore, when a non-consolidated item is presented between two consolidated items, it is relatively "insulated" from interference on both sides—the consolidation process can begin immediately upon presentation of the item and can continue through the presentation time of the following item. The act of paying attention to each item in the list, whether it has been consolidated or not, might, of course, result in some interference, but non-consolidated items would cause more interference than consolidated items.

The fact that the adjacency effect on item i was found to be greater for the preceding item ($i - 1$) than for the following item ($i + 1$) suggests that it is somewhat easier to interfere with the *beginning* of the consolidation process than to interfere with the process once it is underway. If item $i - 1$ has already been consolidated, the consolidation of item i can begin at once and will probably continue until it is complete. Of course, it will continue with less interference if item $i + 1$ has already been consolidated. If item $i - 1$ has not been consolidated on a previous trial ($t - 1$), however, it is apt to be consolidated on trial t and would then overlap the presentation of item i, so that even if $i + 1$ had already been consolidated, the start on item i might come too late to avoid interference from item $i + 2$.

There is also evidence in the present data that the consolidation of an item can interfere with an already consolidated item and "undo" it. In *Analysis 3b* it was found that the following item ($i + 1$) alone produced no adjacency effect *when item* $i + 1$ *is also recalled on trial* t, which of course, means item $i + 1$ did not suffer appreciable interference from item i. But it is most interesting to note the case where item $i + 1$ was recalled on trial $t - 1$ but was *not* recalled on trial t. Here the adjacency effect *did* occur (phi $= 0{\cdot}06$, $SD = 0{\cdot}26$, $t = 2{\cdot}50$, $d.f. = 122$, one-tailed $p < 0{\cdot}01$), which suggests that the consolidation of item i interfered with the retention of item $i + 1$. In other words, it was found that when an item was recalled on trial $t - 1$ and then was *not* recalled on the following trial (t), it was preceded, in the presentation for trial t, by an item (i) that was recalled on trial t. The consolidation of item i (as inferred from the fact that it was recalled) apparently interfered with the retention of item $i + 1$.

According to this hypothesis the rate of presentation of items is a crucial factor in the adjacency phenomenon. The hypothesis might be tested by inserting "blank" spaces of various lengths into the presentation series and noting the effects on the probability of recall of the items adjacent to the "blanks" which presumably would not much interfere with consolidation. Relevance to the von Restorff effect is thus also suggested. But any further speculation about the basic nature of the adjacency effect hardly seems warranted at this stage.

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INPUT RESTRICTION AND IMMEDIATE MEMORY DECAY IN NORMAL AND SUBNORMAL CHILDREN

BY

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Twelve imbeciles and 12 normals, matched for their digit memory span, were presented with three digit numbers successively and simultaneously. Seven speeds of presentation were used. Each simultaneous speed of presentation had a corresponding successive speed. Subjects were required to recall digits on the conclusion of presentation of each number. Each performance was scored as the number of errors for each digit. With simultaneous presentations the difference between the groups was significant only at fast presentation speed, where the normals were significantly better than the imbeciles. Imbeciles improved at slow speeds and became as good as normals. In the case of successive presentations differences between groups occur only in relation to the second digit, which the normals remember better than the subnormals. Both groups remember the first digit worst as rates of presentation become progressively slower. Results are explained in terms of input restriction and of memory trace decay.

INTRODUCTION

In previous studies (O'Connor and Hermelin, 1963), it was shown that long-term memory for well-learned material was not impaired in subnormals as compared with normal controls of like mental age. Memory deficits in imbeciles may thus be due to limitations of initial input rather than to retention deficits. Hermelin and O'Connor (1964), using delay periods between presentation and recall of 2, 6 and 12 sec., showed that whereas normal scores did not decline over time, those of the subnormals did. A significant decrease in memory score from a delay of 2 sec. to one of 6 sec. was found. This decline in the imbecile scores was presumably dependent on decay of memory trace through lack of rehearsal.

To estimate the relative importance of input restriction and decay in memory deficits, imbecile and normal children of the same mental age were compared for their short-term memory of three digit numbers presented visually. The digits were seen either simultaneously or successively, and at seven different display rates. In simultaneous presentations, fast display rates should limit input capacity and consequently reduce overall scores. With long display times on the other hand, all the digits could be read and re-read, thus preventing decay and improving recall. Conversely, in conditions of successive presentation, long display times should mean that the trace of the first digit in a message has had time to decay before the last disappears and recall begins. This would be true only if no rehearsal occurred during this period. Thus fast rates of successive presentation should result in better scores than slower rates. Such a result has been reported by Conrad and Hille (1958) for normal adults. However, Mackworth (1962), comparing the effects of display time on memory scores in conditions of simultaneous and successive presentation, had somewhat different results. In her experiment display times between 0·25 and 1·0 sec. per digit, or 2·5 sec. to 10·0 sec. per message, gave progressively better results as presentation time was increased. This was true of both methods of presentation. Mackworth concluded that her results do not support a decay theory of immediate memory.

Thus two contradictory predictions concerning the present experiment could be made on the basis of previous findings. Following Conrad, who used an auditory and therefore successive method of presentation, fast rates should give better results than slow ones. According to Mackworth, however, using visual successive and simultaneous displays, the reverse would be expected. Our own prediction resolves these two contradictory statements by invoking two mechanisms—input restriction and trace decay. We would maintain that fast presentation rates should cause input difficulties in simultaneous displays and lead to bad results. On the other hand, in successive displays slow rates of presentation should lead to bad results.

METHOD

Subjects

The average MA measured on the Terman Merrill Scale and the CA for the imbecile group was 5/11 ± 0/5 and 14/8 ± 1/2 respectively. Imbeciles and normals of like mental age were matched on their memory span for digits. This was done by presenting single 1 in. high black digits on separate white 3 in. by 2 in. cards at an approximate rate of two per sec. Three, four and five digit numbers were shown to each subject to determine his memory span. An acceptable response was all digits repeated in the correct order. Only those who had a memory span of three digits were selected as subjects. From these, 12 imbeciles aged 14/8 ± 1/2 and 12 normals aged between 5/6 and 6/4 were chosen. Children of abilities above and below this level were eliminated from the experiment whatever their mental age.

As matching was inevitably for one rate and method of presentation, no prediction could be made concerning the performance of different groups at different rates and for the alternative method.

Apparatus

An apparatus was designed to present 48 three-digit numbers. The digits could be presented simultaneoulsy or successively. In the simultaneous presentation all three digits of a number appeared and disappeared together. In successive presentations the first digit of the number appeared and then disappeared as the second appeared next to it, and the second disappeared as the third appeared next to that. Speed of presentation could be varied for either procedure. Each simultaneous presentation had a corresponding successive speed. Each successive digit appeared for one-third of the time of the total corresponding simultaneous display. Table I gives the speeds used in each case from fast to slow.

TABLE I

SPEEDS OF PRESENTATION OF THREE DIGIT NUMBERS (SEC.)

Simultaneous (three digit speed)	0·38	0·50	1·0	2·0	3·0	6·0	10·0
Successive (single digit speed)	0·13	0·17	0·33	0·66	1·0	2·0	3·3

Successive or simultaneous presentation could be predetermined by throwing a switch. Exposure times for any numbers could be pre-set.

In the construction of this apparatus, commercially available one-plane digital display units were used and numbers were presented at eye level and at a distance of 3 ft. Digits were 1 in. by $\frac{5}{8}$ in. in size and had an illumination value of 10 ft. candles in a darkened room. Remote control of a uniselector enabled silent presentation of each display.

Procedure

Each subject took all seven different speed conditions under both simultaneous and successive conditions of presentation. There were thus 14 conditions in all, with eight three-digit displays in each condition. Thus each subject attempted to read a total of 112 three-digit displays. An equal number of subjects received simultaneous and successive methods of presentation first. The order of presentation of speeds was also randomized within procedures. It was realized that the same numbers were seen more than once by each subject but the effect of this deficiency in the apparatus was randomized for speeds and procedures.

Each subject carried out the experiment over two sessions to avoid fatigue. After the display of each number the subject's response was recorded. All such records were later scored for errors. Recall was oral and unpaced.

RESULTS

A first analysis of variance compared simultaneous and successive conditions, rate of presentation and groups. The scores analysed were the total number of messages correctly recalled, i.e. the three correct digits in the correct order. These scores are given in Table II. All the main terms were significant. Normals recalled better than subnormals and simultaneous conditions of presentation gave better results than successive conditions. Fast presentation rates resulted in more errors than slower ones. These results seemed to confirm those of Mackworth (1962) but her results were not scored as the percentage of correct messages but the average number of digits correct per message.

TABLE II

MEAN ERROR SCORES FOR GROUPS, METHODS OF PRESENTATION AND SPEEDS
(Maximum score 8·0)

	Times	0·38/0·13	0·50/0·17	1·0/0·33	2·0/0·66	3·0/1·0	6·0/2·0	10·0/3·33
Imbeciles	Simultaneous	4·8	4·8	4·2	4·0	3·0	2·8	3·0
	Successive	5·6	5·7	4·6	5·0	4·6	5·0	5·0
Normals	Simultaneous	4·0	2·5	3·0	2·1	2·0	2·7	2·4
	Successive	4·7	4·7	4·0	2·2	2·7	3·3	4·1

For subsequent analyses we used a further method of scoring the number of errors per digit by its serial position in the three-digit number. This method was chosen because it seemed most appropriate to the comparison of the effects of the two presentation procedures. It was found empirically that it was difficult to devise a scoring system which was easy to interpret when trying to score for order errors only. Nearly all previously used techniques for scoring order errors confound items and order. Scores given below are therefore errors scored for each individual digit of all eight three-digit numbers for any one speed of presentation in either successive or simultaneous procedures. Incomplete responses were rare. When one or two correct digits only were offered in response, these were assigned to appropriate correct positions. Errors are thus out of a total possible of eight in the two analyses described below.

TABLE III

MEAN ERROR SCORES FOR GROUPS, METHODS OF PRESENTATION, SPEEDS AND DIGITS
(Maximum score 8·0)

	Times	0·38/0·13			0·50/0·17			1·0/0·33			2·0/0·66			3·0/1·0			6·0/2·0			10·0/3·33		
	Digits	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3
Imbeciles	Simultaneous	1·0	2·9	3·7	1·3	2·9	4·4	1·4	2·1	3·6	0·8	2·4	3·3	1·6	1·5	2·8	0·6	1·5	2·4	0·8	1·9	2·3
	Successive	2·3	5·1	3·8	2·5	4·3	4·3	2·1	3·9	2·8	2·3	4·2	2·7	2·5	3·6	2·7	3·1	3·8	2·8	3·4	4·1	3·4
Normals	Simultaneous	0·7	1·1	3·4	0·1	0·6	2·1	0·2	1·1	2·6	0·7	1·1	1·8	0·6	1·1	1·6	0·9	1·0	2·3	0·5	1·5	2·2
	Successive	1·1	3·4	2·8	1·3	3·1	3·9	1·3	3·1	3·3	0·6	1·3	1·4	1·1	1·5	1·5	1·9	2·1	1·9	2·8	2·9	2·4

Mixed Design Analysis of Variance techniques were used, with Groups, Digits and Speeds as the variables (Maxwell, 1958). Separate analyses were carried out for simultaneous and successive presentations. Table III gives average errors as indicated, and Tables IV and V the summary analyses of variance for simultaneous and successive methods of presentation.

TABLE IV

MIXED DESIGN ANALYSIS OF VARIANCE OF GROUPS BY SPEEDS BY DIGITS
Simultaneous Presentation

Source	*d.f.*	V^2	*VR*	*P*
Between groups	1	91·72	16·95	
Between digits	2	164·23	74·55	0·001
Between speeds	6	4·62	2·10	
Groups × digits	2	3·47	1·57	N.S.
Groups × speeds	6	6·37	2·88	0·01
Digits × speeds	12	2·55	1·14	N.S.
Groups × digits × speeds	12	1·54	0·69	N.S.
Between people within groups	22	5·41		
Residual	440	2·22		
Total	503			

TABLE V

MIXED DESIGN ANALYSIS OF VARIANCE OF GROUPS BY SPEEDS BY DIGITS
Successive Presentation

Source	*d.f.*	V^2	*VR*	*P*
Between groups	1	177·38	8·57	
Between digits	2	73·34	28·99	
Between speeds	6	16·33	6·46	
Groups × digits	2	8·69	3·44	0·05
Groups × speeds	6	4·66	1·86	N.S.
Digits × speeds	12	6·69	2·68	0·01
Groups × digits × speeds	12	1·96	0·77	N.S.
Between people within groups	22	20·71		
Residual	440	2·52		
Total	503			

Simultaneous presentation

A significant interaction between groups and speeds was found. Subsequent testing of significant relationships in the simultaneous presentation made it clear that although imbeciles had more errors than normals, this difference between the groups reached significance only at a fast (0·50 sec.) presentation speed. In the slow presentations the differences were not significant. As far as the individual groups were concerned, normals showed similar error scores at all speeds, except that at the 0·5 sec. presentation rate they showed less errors than at other speeds. Imbeciles, however, began badly and improved significantly from the 0·5 presentation speed to slower rates. The above interaction eliminates the need to account for the significant Between Groups and Between Speeds main terms. It remains to account for the significant Between Digits term. Each digit differed significantly from the two others for the combined groups error scores, and errors increased in a relatively linear fashion from the first to the third digit.

Successive presentation

A significant interaction between digits and speeds was found. For the first digit a significant increase in errors occurred at speeds slower than one digit per 0·66 sec. For the second and third digits errors increased both for speeds slower than and faster than one digit per 0·66 sec. This increase in errors is not, however, significant for the slower speeds. The rate of one digit per 0·66 sec. is thus an optimal presentation rate. Total errors increase for slower and faster speeds yielding a U-shaped curve.

The Groups by Digits Interaction is accounted for so far as groups are concerned by a significant t (0·01) between imbeciles and normals in the second digit only which the normals remembered better than the subnormals. First and third digits did not differ between the groups. Within each group first, second and third digits differed significantly for the imbeciles who recalled the first best and the second worst. The normals also recalled the first best but their scores for the second and third did not differ.

DISCUSSION

In the Introduction it was suggested that recall deficits in subnormal children might be explicable in terms of two factors, input restriction and memory decay. Using the sensitive method of scoring individual digits by their position, we obtained some support for this theoretical model. Both groups remembered the first digit best and the third worst in conditions of simultaneous presentation. As predicted, the severely subnormal did better in simultaneous presentations with a long display time, presumably because of limited input capacity. This might be dependent on a slow reading speed for example. Longer displays allowed more time for reading and integrating the digits.

In conditions of successive presentation as previously stated, there was a suggestion of an optimal presentation rate of one digit per 0·66 sec. At faster rates bad recall scores seemed to be caused by input restrictions and at slower rates by memory decay. However, this tendency is significant in the case of the first digit only, which is forgotten at slower speeds of presentation and for the second, and third digits only for faster speeds of presentation, where they do not seem to be taken in and registered.

In a previous study by Hermelin and O'Connor (1964) a time interval of two sec. between presentation and recall was shown to be critical for recall in imbeciles and less so for normals. This finding supplemented studies by Ellis (1963) and his colleagues concerning the importance of the role of trace decay in recall in imbeciles. The results of the present experiment, however, introduced a further factor in recall other than that of memory decay. Indeed decay rates under conditions of successive presentation are similar for normals and subnormals.

In conditions of fast simultaneous presentation, where decay is irrelevant, normals still recall better than subnormals. This indicates that input restrictions operate as an additional factor in imbeciles.

Whatever the interaction of the two mechanisms, both contribute to the loss of information. At faster presentation rates than 2 sec. for three digits, input restrictions operate, but at slower rates than 2 sec. decay is more important.

As predicted the suggestion that normals and subnormals differ in input capacity depends on results obtained in conditions of fast simultaneous presentation. On the other hand, in conditions of successive presentation decay rates are similar for the two groups. In these conditions, as demonstrated by a significant Groups by Digits

Interaction, it is only the second digit which is remembered significantly worse by the subnormals. Presumably this is explicable in terms of greater pro-active and retro-active interference in the subnormals.

In this experiment we were unable to control recall rate because subnormals could not co-operate to this extent. Despite this, we consider that the results justify the following conclusions.

(1) In conditions of simultaneous presentation of digits at fast rates normals remember better than subnormals. There is no difference when presentation rates are slow. This suggests limited input capacity in imbeciles.

(2) When simultaneously presented the first digit is remembered best and the last worst.

(3) Under conditions of successive presentation both groups remember the first of three digits progressively worse at presentation rates slower than one digit per 0·66 sec. This suggests similar rates of forgetting. In fast presentation rates the second and third digits of a three-digit number are subject to input restrictions in both groups.

(4) The data is best explained in terms of two processes, input restriction and memory decay. While decay seems to occur equally in normals and subnormals input restrictions are more marked in the latter.

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THE SIZE-CONSTANCY OF UNDERWATER SWIMMERS

BY

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The size-constancy of divers was measured by requiring them to adjust the distance between two disks of unequal size so that they appeared phenomenally equal. In clear water divers showed greater constancy-ratios than on land. In murky water, where visibility was reduced by suspended particles, constancy-ratios were the same as, or less than, on land. It is suggested that these effects may be due to changes in apparent distance: in clear water objects appear nearer through refraction, but in murky water they appear further away because of the distance cues provided by the visibility gradient.

The orientation of the display, or of the diver's body, did not affect constancy under water, though it does on land. It is argued that the effect on land is due to visual and proprioceptive cues which are absent in the water.

INTRODUCTION

A diver with a snorkel or aqualung can look through his face-mask and swim around exploring a rather strange world, and it is interesting to know how well he can judge size and distance under these conditions. The water may be clear or full of dirt particles, dimly lit or pierced by shafts of light, empty or full of unfamiliar objects. The diver may vary between positive and negative buoyancy, and he lacks some of the usual cues to bodily orientation. Any of these factors might act to confuse, or systematically change, his perception of size and distance.

It is important to distinguish between *optical* and *perceptual* changes of distance. Optical distance can be altered by such things as lenses or the refractive index of water, and can be precisely calculated. Perceptual distance is determined by the various binocular and monocular depth cues, and it is not possible to predict the precise effect that any set of cues will have for all observers. Consequently a given change in optical distance will not necessarily produce the same change in perceptual distance.

Most people are familiar with the optical change of distance of objects seen when looking into water. The refractive index of water is 4/3 that of air, and light rays are bent away from the normal on entering air from water. The result is that an object is located optically at 3/4 of its depth, and subtends a larger angle at the observer's eye, giving a correspondingly larger image (4/3 larger, if the eye is exactly at the surface of the water). The perceptual effect can be observed by comparing a coin in a jar of water with one outside. The effect varies among different observers, but in general the coin appears fairly near its optical depth. If, however, the water is made semi-opaque by the addition of ink, the coin appears further away than in clear water; it may even appear further away than in air, particularly with monocular vision.

A diver normally wears a face-mask, so that the eye operates in air and looks into water, and objects are optically nearer. If the eye is opened in water without a face mask it becomes extremely long-sighted. This is because the primary image-producing lens is the air-cornea interface, the aquaeus humour behind the cornea having a comparatively high refractive index; the refractive indices of water and the aquaeus humour are similar, so that little refraction occurs at the water-cornea interface.

When measuring size-constancy under water it is important to consider the effect of a change of distance upon constancy-ratios. The effect seems to depend upon the method used. If subjects are asked to compare the physical size of a distant object with a near object, there is a tendency towards overconstancy, or the overestimation of the size of the distant object with increasing distance (Epstein, Park and Casey, 1961). If, however, apparent size is stressed, the opposite may occur (Van de Geer and Zwaan, 1963), or there may be perfect constancy (Carlson, 1960). There seems to be no direct evidence concerning the effect of altering the distance of *both* objects in the same proportion, as happens in clear water. The apparent-size match would be unaltered only if there were an invariant relationship between optical distance, apparent distance and apparent size, which is unlikely (Epstein, Park and Casey, 1961).

In the experiments reported here a design of the Thouless (1931) type was used, where the distance of a larger object is varied until it appears equal in size to a smaller nearer object. The purpose of varying distance instead of size (which is more normal) was to make a simple sliding apparatus, which would be unlikely to break down under water. The disadvantage of such a method is that the subject is forced to judge apparent and not objective size, and can never demonstrate perfect constancy. "Apparent size" instructions lead to varied performance between subjects, and it cannot be assumed that the results would necessarily be repeated with "objective size" instructions. The limitations of this method are discussed by Lambercier (1946).

APPARATUS AND METHOD

The apparatus (see Fig. 1) consisted of a 6 ft. long aluminium rod forming an H-section, to each side of which was attached a white dural disk on a stalk and sliding base. One of the disks was 5 in. in diameter, and it was fixed permanently at a distance of 13 in. from the viewing end of the rod. The other disk was 5·5 in. in diameter, and its distance could easily be altered by pulling or pushing a rod which was attached to the sliding base and supported through a hole in a block at the front end of the main rod. The horizontal distance between the centres of the two disks (when equidistant from the end of the rod) was 14·75 in. A 6-in. piece of copper wire, hanging perpendicular to the end block, served as a marker where the subject put his face-mask (or his nose, when on land). A piece of cord 15 ft. long was attached to each end of the main rod, so that the whole apparatus could be suspended from a boat or the side of a swimming pool.

FIGURE 1

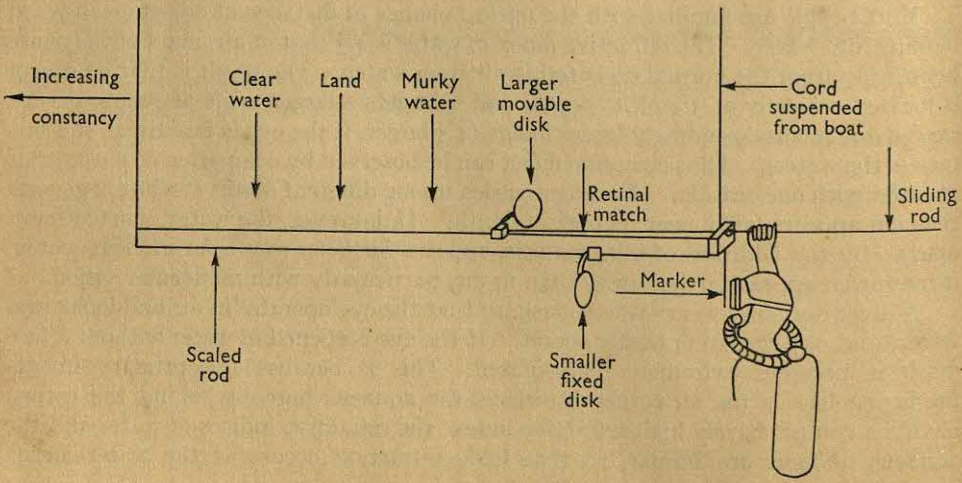

Diagram of apparatus used in measuring size-constancy. The diver adjusts the distance of the larger disk until it appears equal in phenomenal size to the smaller disk. The position of the settings under different conditions is indicated (not to scale).

The subject was instructed to place his face-mask against the marker, look at the two disks with both eyes, and adjust the distance of the larger disk until it appeared equal in phenomenal size to the smaller disk. It was explained that this meant neither equal object size (impossible), nor equal retinal size (which he should not try to calculate), but apparent size, which varied among individuals. The subjects seemed to have no difficulty in understanding the instructions. Since the two disks were on opposite sides of the line of sight of the subject, it was impossible for him to make a retinal match by lining them up in front of one eye. The setting of the larger disk was read by another observer from a tape-measure glued down the centre of the rod.

If the subject were matching the retinal angles subtended by the disks, he would place the larger disk at 14·6 in. from the end of the rod (assuming his eye to be 3 in. behind the marker). If he had perfect size-constancy, he would attempt to place it infinitely far behind the end of the rod. In practice subjects set it between 14·6 in. and the end of the rod, and the settings were taken as a measure of size-constancy. When testing the significance of differences between settings under different conditions, the sign test was used (Siegel, 1956) to avoid any assumptions about the scale values of the differences. The means quoted for groups of subjects are geometric means, to avoid giving undue weight to distant settings. The semi-inter-quartile range (SIQR) is given as a non-parametric estimate of variance.

EXPERIMENT 1: THE EFFECT OF CLEAR WATER

The effect of clear water on size-constancy was tested in a warm indoor swimming pool. The subjects were undergraduates and research students, 14 males and one female, aged between 18 and 28 years. They used snorkels for the experiment, with their heads just below the surface of the water. Each subject made two settings in water and two on land, one setting starting with the large disk at the near end of the rod, and one starting at the far end. Half the subjects were tested first in the water and half on land, and all had a preliminary practice on land. The geometric means of the two settings were taken for each condition for each subject. The settings were found to be significantly further away under water than on land ($p = 0{\cdot}008$, sign test, two tails). The mean setting on land was 39·8 in. from the eye (SIQR 8·0 in.), and 44·3 in. under water (SIQR 7·88 in.)

This difference in setting means that there is a greater constancy-ratio under water. The Thouless ratio is given by

$$\frac{\log \mathrm{P} - \log \mathrm{S}}{\log \mathrm{R} - \log \mathrm{S}}$$

where P = relative phenomenal sizes (here unity)
S = relative retinal sizes
R = relative object sizes

The Thouless ratio on land was 0·8954, and in water 0·9064. This formula would predict no change in ratio under water if the eye were exactly at the surface of the water, since the changed optical distances of the disks would leave the relative retinal sizes unchanged. However, the diver's eye is about 2 in. behind the front of the face-mask, so that there is a constant air-space involved and there is a slight change in the relative retinal sizes. The distances involved are shown in Figure 2. If the under-water constancy-ratio is recalculated on the equivalent optical distances (12·5 in. small disk; 33·73 in. large disk) it becomes 0·9040, which is still slightly too large. If the ratio were to remain the same under water, the larger disk should be set at an optical distance of 31·07 in., or a physical distance of 40·75 in. This means that divers might be expected to place the disk about 1 in. further away under water. Twelve out of 15 subjects showed differences greater than that ($p = 0{\cdot}036$, sign test, two tails). This suggests that more is involved than compensation for the slight relative size change.

Since this method of measuring constancy is dependent upon change of distance, it is probable that an absolute change of distance is important even though the

FIGURE 2

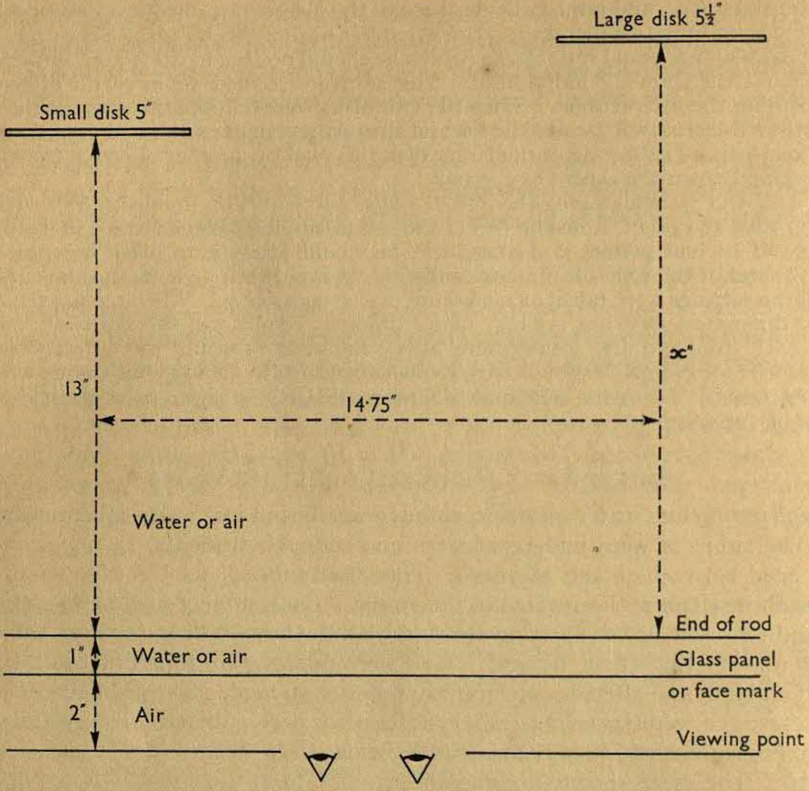

Diagram showing the distances involved in the experiment. *x* in. represents the variable distance of the larger disk from the end of the rod. In air, 3 in. is added to the rod measurement to give the total distance of the disk from the eye. In water, the optical distance of the disk is calculated by taking 2 in. of air (in the face-mask) and adding $\frac{3}{4}$ of the remaining distance (*x* in. + 1 in.).

relative distances remain approximately constant. The problem is complicated by the fact that divers rapidly adapt to the change of size and distance under water, and, after a little mis-reaching, see things as normal. The difference is only obvious if some half-immersed object is compared alternately above and below the water. Once under water, everything is altered in the same proportion, so there is no evidence (except memory) that anything is different. It seems as though "distance constancy" operates, apparent distance tending to become the same on land and under water. Consequently, if apparent distance rather than physical or optical distance is the relevant factor in size-constancy experiments, then there should be little difference between land and water, at least for experienced divers. Alternatively, the changed optical distance may be important despite constancy of apparent distance. Further experiments are necessary on this question.

Experiment 2: The Effect of Murky Water

The distance at which an object can be seen under water is affected by absorption and scattering of reflected light from the object, and the veiling brightness of the intervening water due to light scattered in the direction of the eye from outside the light path. The distance at which a black object can be seen is a measure which can be converted into an attenuation coefficient (Duntley, 1960, 1962). In the present experiments visibility was estimated by the distance at which the white disk disappeared when viewed horizontally. The white disk (like a Secchi disk) has higher contrast than a black object, and the estimates are longer: when measured on the same occasion in the sea, 7 ft. white disk was found to correspond to 6·5 ft. black object.

In daylight dives near the surface of the water visibility is mainly limited by light scattering particles. The effect is similar to a fog. In a fog objects generally appear to "loom up large." There may be a slight optical increase in the size of white objects, due to the scattering of light from bright objects against a darker background (Helmholtz, 1856), but this cannot explain the apparent increase in size of all objects. The probable explanation is that the sharp visibility gradient of the fog is taken as a cue to distance. Consequently, objects are scaled in size as though they were further away than they actually are. Fog affects apparent motion in a similar way: objects appear to approach surprisingly fast, because they cover an apparently longer distance in the same time. In murky water the optical effect should make objects appear nearer, while the visibility gradient should make them appear further away. The resultant apparent distance depends upon the observer and the visibility in the water. If an object is compared above and below murky water, it does not usually appear nearer (as it does in clear water), and often appears further away than in the air.

The effect of veiling brightness on constancy-ratios on land was investigated by Murray Martin and Pickford (1938). They imitated a fog by reflecting diffuse light into the eye, and found that a strong glare reduced constancy-ratios almost to zero. They argued that this was because in their experiment the veiling brightness reduced the distance cues. However, as an object becomes more distant in real fog, or under water, the veiling brightness between the object and the eye increases whilst the image-forming light from the object decreases. The resulting decrease in contrast between the object and its background can be used as an additional distance cue. There seems no reason to suppose that the effects observed by these authors would necessarily be found under water.

The effect of murky water was investigated in the sea in March at Martin's Haven on the Pembrokeshire coast, when the visibility was about 7 ft. The apparatus was lowered over the side of a boat, and the diver adjusted the disk while wearing an aqualung, at a depth of about 6 ft. Conditions were so unpleasant (the water was about 7° C.) that it was only possible to obtain one reading from each subject, which was always made with the large disk starting at the near end. One land setting was also obtained. The subjects were 12 male undergraduates or research students, aged between 18 and 28 years. The mean settings were 47·2 in. on land (SIQR 3·0 in.), and 40·9 in. in the sea (SIQR 3·25 in.). The difference was significant ($p = 0{\cdot}002$, sign test, two tails), showing that in very murky water constancy-ratios are lower than on land.

With a medium degree of visibility constancy-ratios may be the same in water as on land. This was found when performing the experiment in Blenheim Lake, Oxfordshire, and Coldham's Lane Gravel Pit, Cambridge, in May, when the visibility was about 15 ft. The subjects were members of diving clubs aged between 19 and 35 years, six males and two females. Each subject made two settings on land and two in water, one setting starting with the large disk at the near end of the rod, and the other at the far end. No significant differences were found. The mean settings were 39·5 in. from the eye on land (SIQR 10·8), and 39·2 in. in the lakes (SIQR 11·6).

The relative positions of the settings on land and in clear and murky water are indicated in Figure 1. The absolute setting on land differs considerably between the sea group (47·2 in.) and the bath and lake groups (39·8 in. and 39·5 in.). This seems to be due to individual differences in the subjects: the SIQR in the latter two groups is high.

The results found in murky water are similar to those of Murray Martin and Pickford, but the reason for the reduced constancy-ratios is not clear. It is more likely to be due to an increase in apparent distance than to an absence of distance cues. There is no reason to suppose that the apparent distance of the two objects increases in proportion, and if it is disproportionate it would upset a constancy match. It would also make it difficult for a diver to learn distance-constancy in murky water. A possible explanation of the results is that, with the apparatus used, the subject was mainly concerned with reproducing an apparent distance; if distance-constancy were incomplete, the same land distance would be represented by a shorter distance in murky water and a longer distance in clear water.

EXPERIMENT 3: THE EFFECT OF ORIENTATION OF THE BODY AND THE DISPLAY

In the previous experiments the subject viewed the horizontal display from an upright position looking straight ahead. However, it is known that in some cases constancy is different on land when looking upwards. An example of this is the moon illusion, when the moon looks smaller in the zenith than on the horizon. Holway and Boring (1940) held that this was due to the raised angle of the eyes in the head, which altered the normal convergence cues to distance. Kaufman and Rock (1962) argued that the effect was too small to account for the illusion, and the illusion was due to the presence of distance cues on the horizon which made the moon appear further away, and thus larger by size-constancy. The difficulty with this explanation is that most people report that the moon appears *nearer* on the horizon. Gregory (1965) suggests that the illusion may be partly due to the presence of perspective cues on the horizon, which act directly to increase apparent size independently of apparent distance. It also seems likely that proprioceptive information affects size-constancy. Van de Geer and Zwann (1963) have shown that, even when all distance cues are identical, constancy-ratios are reduced when looking vertically up or down; they argue that this is due partly to the angle of the eyes in the head, and partly to proprioceptive information concerning the orientation of the body (and hence of the display). Further evidence on the interaction of proprioceptive and visual information is given by Witkin (1952) in studies on the perception of the upright, and by Gregory and Ross (1964) in studies of size-constancy during movement of the observer.

In general, the evidence suggests that when looking upwards objects appear further away and that constancy-ratios between two objects in that plane are reduced; but that when comparing an object in the vertical plane with the same display in the horizontal plane, the object appears paradoxically smaller in the vertical plane; and that the addition of extra perspective cues on the horizon may increase the illusion.

It is interesting to see whether constancy differs in different orientations under water. In clear open water the distance cues are the same in all directions, except that the surface is brighter. Proprioceptive information from the joint receptors and pressure receptors is greatly reduced, though vestibular cues are still present. Marked decrements in the knowledge of the vertical under water have been reported (Loftus and Hammer, 1961).

Testing was carried out in the sea at Martin's Haven in June, when the visibility was about 25 ft., at a depth of about 15 ft. The apparatus was lowered either horizontally or vertically from a boat, and the subject was asked to orientate himself so that his body was in line with the apparatus or at right angles to it. There were thus four positions: rod horizontal, subject vertical (HV); rod and subject horizontal

(HH); rod and subject vertical (VV); and rod vertical, subject horizontal (i.e. on his back, looking up) (VH). In the HH and VV positions the subject tilted his head back and raised his eyes in order to see the disks, while in HV and VH he looked straight ahead. Five subjects were used, four males and one female, undergraduates and research students aged between 18 and 28 years. Each subject made four settings in each orientation, in a random order, always starting with the disk at the near end. They also made four settings on land in the normal HV position. The means of their settings are shown in Table I. There was no evidence of any effect due to the different orientations; the mean setting was in each position near to 37 in. The only significant difference was that the land settings were lower than the sea settings, with a mean of 34 in. ($p = 0\cdot031$, sign test, one tail). This was expected because of the effect of clear water.

TABLE I

THE GEOMETRIC MEANS OF FOUR SETTINGS ON LAND AND IN DIFFERENT ORIENTATIONS IN THE SEA, IN INCHES FROM THE EYE. FOR EXPLANATION SEE TEXT

Subject	*Land*	*Sea*				*Sea mean*
	HV	*HV*	*HH*	*VV*	*VH*	
1	43·50	42·77	48·26	42·93	44·34	44·51
2	33·70	36·51	33·80	33·94	33·46	34·41
3	28·83	32·71	36·46	38·94	39·90	36·89
4	34·27	40·15	37·36	37·14	33·90	37·07
5	31·34	34·97	35·58	34·17	34·22	34·73
Mean	34·00	37·25	37·98	37·28	36·93	37·36

There are no significant differences between any of the orientations in the sea.

The land settings are significantly lower than the means of the sea settings ($p = 0\cdot031$, sign test, one tail).

A control experiment was performed in the laboratory to ensure that it was possible to obtain significant orientation effects using this apparatus and method. Five male undergraduates aged between 19 and 22 years were used as subjects. The same procedure and orientations were used as in the water. The testing was done in a laboratory room in daylight, and no attempt was made to remove distance cues. For the positions in which the subject was horizontal he lay on an airbed on a table, on his back or front. For the other two positions he stood upright. The far end of the rod was held by the experimenter, while the subject held the near end and adjusted the disk. The results of this experiment are shown in Table II. Fairly consistent position effects were found, significant at the 0·01 level on the Friedman two-way analysis of variance (Siegel, 1956). There seems to be no effect due to the angle of the eyes in the head, but a large effect due to the orientation of the display. With the rod horizontal the mean setting was 55·9 in., while with the rod vertical it was 45·9 in., showing that there are reduced constancy-ratios while looking upwards regardless of the orientation of the subject's body. There also seems to be a small effect due to bodily orientation, since (for the same rod orientation) subjects show lower ratios if they are lying on their backs or fronts than if they are standing upright.

There is a large difference between the HV land settings of the sea and laboratory groups (34·0 in. and 57·31 in.). This may be because there were only five subjects in each group, who happened to differ widely; or it may be because there were many more distance cues in the laboratory than there were on the open beach, thus improving constancy-ratios.

Since orientation effects can be clearly shown on land, their absence in the water must be due to the absence (or marked reduction) of the usual visual and postural cues to orientation. This lack of cues can be dangerous, since divers have occasionally been known to drown by swimming downwards instead of upwards. When uncertain about their orientation, divers rely on visual or tactile cues such as the way heavy articles hang, or the direction in which their waste air bubbles float; they may also know whether they are negatively or positively buoyant, and (provided they are not too deep) can detect pressure changes in the ears with change of depth. Knowledge of the upright in water is thus rather different from on land, and it is not surprising that it fails to affect size-constancy in the same way.

TABLE II

THE GEOMETRIC MEANS OF FOUR SETTINGS IN DIFFERENT ORIENTATIONS ON LAND, IN INCHES FROM THE EYE. FOR EXPLANATION SEE TEXT

Subject	*HV*	*HH*	*VV*	*VH*
1	57·09	48·52	44·01	42·30
2	61·25	60·36	55·08	34·77
3	59·17	61·20	53·11	50·08
4	63·59	59·23	46·95	57·10
5	46·96	45·16	41·18	39·48
Mean	57·31	54·48	47·78	44·07

Mean for eyes normal (HV + VH) = 50·25.
Mean for eyes raised (HH + VV) = 51·02.
Mean for rod horizontal (HV + HH) = 55·88.
Mean for rod vertical (VV + VH) = 45·88.

The Friedman two-way analysis of variance shows that there is some effect due to the different orientations ($p < 0·01$).

I should like to thank the members of the Cambridge University Underwater Exploration Group and the Oxford Underwater Research Group who kindly gave up diving time to take part in these experiments. I should also like to thank Mr. R. Ward for designing and building the apparatus, and Mr. R. L. Gregory, Dr. J. N. Lythgoe, Mr. C. C. Hemmings and many others for helpful advice and criticism. This work was carried out while the author was holding a D.S.I.R. research studentship.

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SHORTER ARTICLES AND NOTES

INTRA-LIST DIFFERENCES IN SHORT-TERM MEMORY

BY

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Subjects attempted to recall lists of nine consonants immediately after presentation. Using various recall orders it was found that recall of part of a list interfered with retention of the other parts, memory for items presented early in a list being less affected by such interference than was that for later items. Since this result was not obtained when rate of presentation was increased, it is suggested that rehearsal contributed to the greater stability of early items.

INTRODUCTION

In experiments on short-term memory it has been observed that a relatively higher proportion of the material is correctly remembered when recall of part, rather than all of the originally presented material is required. For instance, Anderson (1960) presented subjects aurally with lists of 12 digits at the rate of one item per second, and found that if after presentation of a list she requested recall of any one of the first, second and third groups of four digits constituting the list, recall averaged about 77 per cent., whereas when all 12 digits were required about 64 per cent. were recalled correctly. A similar result has been obtained by Harrison (1964). Additional evidence for the importance of recall strategies in the short-term retention of verbal material is provided by Kay and Poulton (1951), Brown (1954) and Broadbent (1957).

By way of explanation it has been suggested that the response of recalling part of the material interferes with retention of the rest. An item in a list may be initially stored at the time of presentation, but such storage may be insufficient to withstand the disturbance caused by recall of other parts of the material. The exact nature of this response interference is not known, although Broadbent (1963) suggests a perceptual factor to be important. It is possible that in the process of being recalled, part of a message re-enters the initial store and affects its contents. Whatever the precise nature of the interference, it is agreed that recall of part of a sequence of items being stored over a short period may adversely affect recall of the rest of the sequence, and, therefore, that order of recall will have an effect in determining what part of a list is most likely to be remembered.

Immediately after the presentation of a list, recall is not equally good for the various parts. Harrison (1964) found that when a list of 12 items presented at the rate of one per second is followed by a cue for partial recall, subjects are better at remembering the last four than the first four items. It is likely that presentation of later items interferes with the storage of early items. On the other hand, when presentation rate is slow some rehearsal is possible, which might favour the early items. The total amount of recall of a whole list is also affected by order of recall (see, e.g. Broadbent, 1957). This may be because recalling items does not affect the retention of all other parts of a list equally, as is suggested by the results of an experiment by Tulving and Arbuckle (1963). They varied the sequence in which paired-associate items were to be recalled, and found that when items presented last were to be recalled first, retention of them was practically perfect, but that whereas retention of items presented early in a list was relatively little affected by their position in the recall sequence, retention of items presented late varied systematically with their recall positions. It is known that old memories are more stable than new ones (Jost's Law), and Tulving and Arbuckle's results suggest that this dictum holds good over comparatively short periods of retention. In their experiment time between presentation of an item and the request for its recall was between 3 and 60 sec. One purpose of the present experiment was to discover whether this result could be extended to remembering over even shorter periods. If it can, then the question arises of how order of recall should be related to order of presentation for maximum efficiency in short-term retention.

METHOD

The method used in this experiment was similar to that of Anderson (1960) in that following presentation of a list of items subjects were requested to recall separately each third of the total number of items making up the list. However, in the present experiment recall was typically required of all three thirds of the list, in the six possible orders. Immediately after presentation of the list a light, as a cue, indicated which third of the list was first required to be recalled. After the subject has attempted to remember the relevant items, a second, and usually a third light, indicated which parts of the list were subsequently required.

Materials

Lists of nine consonants were presented by tape recorder over a loudspeaker at the rate of one per sec. The letters C, N, P, S and W were not used, the latter because of the length of time required to pronounce it, the others because of their high confusibility with other consonants. Subjects were informed that no letter would be used more than once in any list. Otherwise the lists were random, except that sequences of letters immediately adjacent in the English alphabet, such as G, H were not allowed, nor were sequences which made English words, such as T, R, Y.

A battery of four lights was placed in front of each subject. Not more than one light was illuminated at any time. Three white lights in a line represented the different thirds of three letters which made up each list. For instance, if the list presented was H, D, X, M, K, T, Q, F, Y the left light was a cue for recall of H, D, X, the right light requested Q, F, Y and the middle light, M, K, T. The fourth light which was yellow was placed below the line made by the others, and indicated to a subject that no further recall of the current list was required. It was not used after lists for which all three of the upper lights were shown.

Conditions

There were six conditions, representing the possible orders of recall of the three groups of three letters making each list. Thus, if order of presentation can be represented as 1, 2, 3, the numbers standing for the first, second and third parts of three letters, recall order could be any of the six combinations of the numbers 1, 2 and 3.

Procedure

Twelve subjects, 11 male and one female, all students of the University of Sheffield were each tested individually.

Subjects listened to each list of consonants. Immediately after the ninth letter had been spoken, one of the three upper lights was illuminated. This indicated to the subject which part of the list he was required to recall first. The experimenter recorded the subjects' spoken reports. If uncertain of a part, subjects were advised to make an attempt, unless they had no idea whatsoever of any of the required letters. If they knew some but not all of a part, subjects were asked to indicate the position of the letter being recalled, for example by saying "Blank, G, Blank."

Immediately after a subject had finished attempting to recall the first requested group of three letters, the first light was extinguished, and a second light turned on, indicating which part of the list was next to be recalled. If a subject had attempted no response at all to the first light, the lights were changed after 3 sec.

After illumination of the second light, the third light might, in a sense, be considered redundant, since there was only one remaining part of the list to be required. In order to dissuade subjects from any tendency to make use of this knowledge, for example by quickly rehearsing to themselves the third to-be-recalled part of the list before dictating the second group of letters, the experimenter introduced an extra condition which was used randomly at four intervals in each trial of thirty lists. In this condition, instead of the third aligned light being illuminated, the yellow light was used, indicating that no further response was required for the current list. Subjects' responses in this condition were not used in scoring performance.

Each subject was presented with 60 lists in all, in two trials of 30 lists, which were separated by an interval of 5 min. There were additional pauses of approximately 1 min. after the fifteenth and forty-fifth lists. Each trial contained 24 test lists, four in each of the six conditions, arranged randomly. The remaining six lists in each trial were assigned to the pseudo-condition described above. Intervals of 15 sec. occurred between the recall of each list and the presentation of the next. Before the test trials there was a practice session which consisted of 10 lists. The results indicated no significant differences in scores between the two test trials.

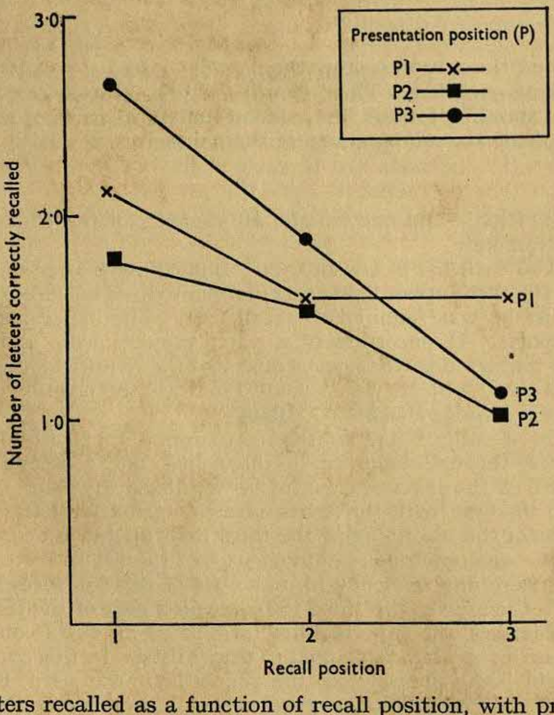
Scoring

Each group of three consonants was scored between 0 and 3, depending on the number of letters that were correct. To be scored as correct a letter had to be in the right position. If no attempt was made to recall any third of a list, that part was scored 0, and any subsequent parts of that list were not scored. For example, if the correct list in the order of recall was GMT LHR BZD, and a subject's attempt was G–H— — —BZD, the scores for the successive parts would be 1/3, 0/3, 0/0. Thus attempts to recall the second- and third-requested parts of a list were only scored when they could legitimately be said to be subject to any effects of the recall of other part(s) of the list. In fact, when the above method of scoring was compared with one in which all attempts to recall any parts were scored, regardless of whether an attempt had been made to recall previously requested parts, differences in scores were small and not significant.

RESULTS

Figure 1 shows the average number of consonants recalled out of three as a function of presentation position, with position of recall as parameter. It is apparent that at the first recall position the last three items in a list are best remembered, whereas by the third recall position retention is best for the first part of a list. This is in line with the findings of Tulving and Arbuckle, and indicates considerable differences in the amount of resistance to interference over very short retention periods (between 1 and 9 sec.).

FIGURE 1



Number of letters recalled as a function of recall position, with presentation position as a parameter.

t-tests showed that the difference in recall of the early items due to response interference (P1R1–P1R3 in Fig. 1) is significantly less ($p < 0{\cdot}01$) than the decrement to the last three items (P3R1–P3R3). Similarly P2R1–P2R3 is significantly less than P3R1–P3R3 ($p < 0{\cdot}001$), and P1R1–P1R3 is less than P2R1–P2R3 (*N.S.*).

Table I gives the average total number of consonants correctly recalled for each of the six recall orders. It is found that maximum efficiency obtains when the thirds of a list are recalled in a reverse order to that of presentation.

TABLE I

MEAN RETENTION OF WHOLE LISTS AS A FUNCTION OF RECALL ORDER

Condition	*Recall order*	*Average number of consonants correct (out of 9)*
1	123	5·00
2	132	4·80
3	213	4·68
4	231	5·29
5	312	5·56
6	321	6·12

t-tests for small correlated samples indicate significant differences between:

6 and 1 ($p < 0·01$).
6 and 2 ($p < 0·01$).
6 and 3 ($p < 0·01$).
5 and 3 ($p < 0·001$).

DISCUSSION

The results suggest that differences in resistance to interference between items may occur within the time needed for presentation of a whole list. An experiment was carried out to examine the suggestion that rehearsal contributed to the apparent greater stability of early items in lists. Results of a similar experiment have since been published (Posner, 1964, Experiment 2). His subjects listened to lists of eight digits at two rates, 60 and 90 per min. They attempted to recall the items either in the original order or saying the last four items before the first four. Posner found that such reversal of recall order improved performance at the slow rate only. The same result was obtained by the present author, who presented nine-consonant lists at rates of 60 and 120 items per min. Recall orders were similar to the 1,2,3, and 3,2,1 conditions of the main experiment, and were indicated by an arrow pointing to the right or to the left. Examination of his results leads Posner to suggest that, "with presentation at the slow rate, rehearsal during the first four items preserves them during the subsequent presentation and recall of the last four." On the other hand, it has been pointed out to the author that it is fallacious to argue that because increasing the time available for rehearsal leads to increased stability in retention, the latter is necessarily *caused* by increased rehearsal. Increasing storage time might by itself lead to items becoming more thoroughly consolidated. However, there is additional evidence to support the point of view that rehearsal at least contributes to increased stability.

(1) When rate of presentation precludes concurrent rehearsal, as in the "fast" condition of the second experiment described, early items are not less affected by interference-due-to-recall, although at the outset of such interference early items will have been retained for approximately twice as long as late items.

(2) Total recall of a list of items, to which stability of retention contributes is usually found to vary positively with presentation time, *except* when rehearsal is controlled by pacing both stimuli and responses. (See Posner, 1963, pp. 334–5.)

It is observed that consolidation may begin to occur very shortly after presentation of the to-be-retained material. It follows that either the mechanisms underlying retention over very short periods are in this respect similar to those subserving long term memory, or that part of the to-be-retained material may be transferred from a brief initial store to more permanent storage within a very brief time from presentation. The latter description is more closely in line with current thinking in this field. (See, e.g. Broadbent, 1958.) There has been a tendency to consider that whereas memory over long periods cannot be examined without reference to "active" mental processes, short-term memory can be explained in terms of storage systems alone. The results of the present experiment help to illustrate the limitations of regarding memory in this way, even over very short periods and where "meaningless" materials, such as random sequences of letters of digits are used. It is clear that (*a*) retention involves material being transferred from one store to another, the transfer being dependent on factors other than the properties of the stores, and that (*b*) transfer of all the contents of the initial store need not occur simultaneously, so that to speak of short-term or long-term memory where the distinction is based solely on the basis of the period of time over which the material is retained is misleading.

The experimental work was carried out while the author held a D.S.I.R. Research Studentship. Considerable assistance by Professor H. Kay is gratefully acknowledged.

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THE COMMON VALUE OF PAUSING TIME IN SPONTANEOUS SPEECH

BY

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Experiments have shown that hesitation increases both before and during speech concerned with the solution of verbal tasks which involve an increasing degree of syntactic or semantic complexity. These experiments have been concerned with differences between tasks. An attempt is made to compare hesitation time between individuals who are set the same tasks, in terms of the quality of their responses to the tasks. Individual dispositions to a characteristic speech-silence ratio complicate such comparisons. Here a strong trend was found between the quality of the subjects' responses and the extra time they took over and above their characteristic speech-silence ratios.

INTRODUCTION

Time as a dependent and a quantifiable variable has been used in several studies of speech as an indicator of cognitive activity. Experiments have shown that hesitation time is involved in lexical as well as semantic decisions when generating spontaneous speech (Goldman-Eisler, 1958, 1961) and that time is also taken up by syntactic transformations (Miller, 1962, 1964).

Time in the above studies is compared as between tasks. Broadly they indicate that the more complex the activity the longer the time involved. Individuals' results were pooled in order to test the generality of the findings. But relatively little research has compared individuals in the use they make of hesitation in speech. Goldman-Eisler (1961) found a significant relation between the tendency of individuals to hesitate when generating speech that required complex cognitive activity and their tendency to produce more or less concise statements in response, while fluent speakers under these conditions were long-winded. But no other work on these lines is known to us, and the reason may be the large variability in hesitation observed between speakers.

In studies of hesitation pauses in spontaneous speech elicited in response to a given verbal task, variation in the amount of pausing between speakers was shown to be both large and consistent (Goldman-Eisler, 1956). Also Miller and McKean (1964) mention the difficulty they had in obtaining stable averages because of the variability in data obtained from different subjects, although they say that part of this may have been due to their search task. Further, an analysis of variance based on our recent data, which is mentioned later, shows that the variance between individuals is highly significant. The measure used in this analysis of the data was pause time in seconds per word uttered (P/W). This gives a reliable estimate of the speech/silence ratio of a spontaneous utterance.

Thus speakers cannot be directly compared in respect of pause measures taken from speech obtained in response to a verbal task, for these measures seem on the face of it to be confounded by individual dispositions to characteristic hesitancy in speech. The size of these may conceivably be governed by an hundred and one variables and may, like height, be randomly distributed in a population.

A previous paper (Goldman-Eisler, Skarbek and Henderson, 1965), was concerned with the relationship between changes in the magnitude of the pause measures across separate drug conditions, and changes in the quality of statements involving generalisation made in response to cartoon tests given in these experiments. The pause measures were taken from the subjects' verbal response to the cartoons.

PROBLEM

The present question is whether or not pause time is comparable across individuals in terms of the quality of the associated utterance? If one imagines that several speakers all of whom are given the same task give replies which are judged as similar in terms of some criterion of quality, would it be expected that their pause measures would be equal? Or, if the replies were judged as different in quality, would the associated pause measures follow an orderly related pattern? In short, does a unit of time have a common value between speakers? Intuitively it might reasonably be argued that such a relationship is unlikely, that some speakers can produce high quality responses with less hesitation than others. But do they? And what is the pause time measure that is to be compared between speakers?

The problem arose in the context of an experiment (Goldman-Eisler, Skarbek and Henderson, 1965) in which the effect of psycho-active drugs upon speech was investigated. Here we will only treat the data obtained under the No Drug condition.

THE EXPERIMENT

Verbal tasks were set in such a way that two levels of verbal activity could be separated in terms of cognitive complexity. Ten subjects were shown a series of six caption-less cartoons (taken from the *New Yorker*) at each session and given the following printed instruction:

> "You will be shown a series of cartoon stories with no verbal captions. You are asked to have a good look at them. As soon as you see the point say 'Got it' and proceed to describe the cartoon story picture by picture; conclude by formulating the general point, meaning or moral of the story in as concise a form as you can. There is no time limit and you may keep the pictures to look at all through the experiment."

The description involves a relatively simple level of speech generation when compared with the verbal task of generalizing from a specific set of events. Using such material Goldman-Eisler (1961) has shown that the relative hesitation in these two situations is significantly larger for the generalizations. (The present results confirm that finding.)

The data was derived from the experiment mentioned above. In this there were three conditions, No Drug, Chlorpromazine and Sodium amytal (the last used as a control for the general hypnotic effects not specific to Chlorpromazine). These were alternated to balance order effects.

TREATMENT OF RESULTS

Only data obtained from the No Drug condition are used here. Mean pause per word measures were taken separately for the descriptions [P/W(D)] and for the generalizations [P/W(G)] for each individual's six cartoons. Pause time is measured by feeding the output from a tape recording into a device described elsewhere (Goldman-Eisler and Mendoza, 1965) which in turn feeds into an electronic counter. This intermediate system translates the signal from the recording into pulses co-extensive in time with the duration of either the speech or the silence periods. Either of these may be selected for feeding to the counter.

With 10 subjects and three conditions, with six cartoons for each condition, there were 180 general statements about the point of the various cartoons. This was the material that three judges (Academic Staff from University College London) were asked to rate according to the criterion set out below. The statements were typed out on cards and arranged so that those referring to the same cartoon were grouped together. The balanced experimental design used in collecting the data ensured that this judging order was also balanced. The judges were asked to rate the statements on a four-point scale according to this criterion:

Generalization. Is the statement a recoding, or a reformulation in general terms so as to be inclusive of a larger variety of specific instances.

The ratings for each statement under each condition for each individual were summed so that each individual for any given condition has a score from each of the three judges representing the degree to which his statements were judged according to the criterion stated.

A high degree of concordance between the judges was evident. Using Kendall's Coefficient of Concordance W = 0·894 which was significant using the Chi-square test with 9 *d.f.* at beyond $p = 0{\cdot}01$ ($\chi^2 = 24{\cdot}14$: $p = 0{\cdot}01$, $21{\cdot}67$: $p = 0{\cdot}001$, $27{\cdot}88$).

In the present experiment only the No Drug data are of interest. An analysis of variance of both the P/W(D) and the P/W(G) measures analysing conditions against individuals, in both cases shows the variance attributable to between individuals to be highly significant (p is considerably less than 0·001). It seems likely then that characteristic dispositions to pausing in speech (P/W) could obscure any relationship that might exist between the quality of the generalisation and pause time.

This was investigated by ranking the individuals' P/W(G) measures from least to greatest and taking this as the predicted order, the generalization scores that the three judges credited each individual with were laid off against their associated P/W(G) measures. Using Jonckheere's (1954) test of trend no systematic increase in generalisation scores was evident as P/W(G) increased. ($p = 0{\cdot}2045$).

However, if the P/W(D) is subtracted from P/W(G) a measure is obtained which may be considered as the P/W increment necessary to produce the generalization. The pause measure P/W(D) obtained from descriptive speech may be thought of as expressing an individual's characteristic speech/silence ratio in these conditions. The P/W increment is thought of as referring the individuals to a common baseline. Comparability between individuals is now possible and meaningful in terms of the extent to which they make use of the extra time they need to produce a generalization relevant to the cartoons described.

The P/W (increment for the ten subjects were then treated in the same way as P/W(G). They were ranked from least to greatest and this order treated as the predicted order of excellence for the generalizations. The three judges' ratings were laid off against their associated P/W (increment) and analysed using Jonckheere's test of trend. The relationship was significant $p = 0{\cdot}019$.

Thus when allowance is made for what has been called characteristic speech/silence ratios pausing time is now seen to be related to the quality of the utterance. Individuals who produce better generalizations tend to take more extra time over and above their characteristic speech/silence ratio, than those individuals who produce inferior generalizations. This then is the time that may be thought of as having a common value between speakers.

Discussion

These results give a picture of an increasingly strong relationship between pause time and generalization as the pause time measure becomes more specific to the task and free of individual dispositions to characteristic speech/silence ratios.

It seems reasonable to suppose that whatever it is that underlies the differences in hesitation in speech between individuals, when these are held constant, the time subsequently involved in speech associated with a verbal task of the sort described, is intimately connected with the quality of the speech product and that furthermore this time is comparable in terms of what is produced as between individuals.

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THE EFFECT OF INSTRUCTIONS ON THE APPARENT REVERSAL OF ROTARY MOTION IN DEPTH

BY

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This study concerns experimental investigation of the effect of instructions on the apparent reversal of rotary motion in depth. It was found that subjects instructed to report apparent reversals of an elliptical shape signalled more reversals than subjects who were instructed to report the non-occurrence of reversals. This difference increased over four trials. When the stimulus object was a trapezium window instructions to report reversals led to a higher rate of reversals than did instructions to report non-occurrence, but this difference did not increase significantly over trials.

INTRODUCTION

In a series of recent experiments (Day and Power, 1963, 1965; Power, 1964) it has been demonstrated that the apparent reversal of rotating shapes is a function of the equivalence of the retinal projections for the rotation and oscillation of the object. That is, the horizontal retinal projections for the two directions of motion (clockwise and counterclockwise) are identical and thus perceptually ambiguous. In the absence of information for depth, therefore, perception of motion direction is largely a matter of chance; an observer may perceive the object as rotating sometimes clockwise and sometimes counterclockwise. The retinal equivalence of the two directions of rotary motion has been quantitatively analyzed by Graham (1963).

In his experiments on the autokinetic phenomenon Sherif (1936) showed that instructions could modify reports of the apparent motion of an ambiguous stimulus (a light source with no frame of reference). It seems reasonable to suppose that the perceptually ambiguous stimulation considered in this paper would be affected by the nature of the instructions. There is some evidence for this. For instance, it has been shown (Day and Power, 1963) that over two or three 20 revolution trials the frequency of apparent reversals tends to increase. It can be argued that as the stimulus conditions are ambiguous this progressive increase is a function of instructions to report reversals (rather than non-reversals). That is, reporting reversals fulfills the instructions and is in this sense reinforcing. Evidence for this suggestion is to be found in the experiments by Cappone (1963), McGee (1963) and Mulholland (1963) who have shown that instructions can affect reports of apparent motion of rotating objects. However Cappone and McGee told subjects that they were perceiving too few reversals for their age group, and Mulholland asked his subjects to try to see certain types of motion.

The purpose of the experiments reported here was to establish the effects of instructions for reversal and non-reversal (complete rotations) on the frequency of apparent reversals. In Experiment I the effects of instructions on apparent reversal of a rotating ellipse were examined. Half of the subjects were asked to report the occurrence of rotary motion, whilst the other half were asked to report the occurrence of apparent reversals. At no stage was it suggested that they should see, or try to see, any particular type of motion. Experiment II was essentially similar, except that the stimulus object was a rotating trapezium "window."

APPARATUS

The apparatus was housed in a semi-darkened room 12 ft. by 12 ft. It consisted of a box-like frame covered with black cotton velveteen to exclude exterior light. The ends were of hardboard 2 ft. wide and 3 ft. high, and were 5 ft. 2 in. apart, and together with the supporting wooden struts were painted matt black. Projecting from one end of the box was a viewing tube 8·5 in. long and 3 in. in diameter beneath which was an adjustable chin rest. At the viewing end of the tube was a circular stop with a hole 2 in. in diameter which reduced the field of vision to about 18°.

The driving apparatus was a synchronous motor, which rotated the shapes at 4·7 r.p.m. The rod on which the shape was mounted could be inserted into a socket on top of the gear box and held in place by means of a thumb screw. When mounted, the rod holding the shape was 52 in. from the subject's end of the viewing tube and 1 ft. from either vertical side of the box. The shapes occupied the centre of the subject's field of vision. A screen 8 in. high concealed the motor and gear-box from the subject. A cam on the shaft supporting the shape closed a microswitch once on each revolution which deflected one pen of a two-channel paper recorder. An audible click was produced when the microswitch closed, but as this was constant for the two conditions in each experiment it was concluded that it would not have any differential effect on the results. The shape was set so that the switch closed when the shape moved through the sagittal plane. The other pen was activated by a press-switch held by the subject. The paper records enabled the experimenter to determine the actual position at which the subject reported the shape to reverse. An occluding shutter mounted just inside the box could be raised or lowered by the experimenter.

The box was illuminated by a 40 watt circular fluorescent tube 18 in. in diameter, mounted on the front ceiling of the box, and tilted towards the stimulus object so that it made an angle of 30° with the horizontal. The shapes, when in the frontal plane had a luminance of 2·53 foot lamberts.

All subjects in Experiment I observed an elliptical shape cut from 1/16 in. aluminium sheet and painted matt white. The major (vertical) axis was 10 in., the minor (horizontal) axis was 6·25 in., and the shape was mounted on clear perspex rod 0·073 in. in diameter. The subjects in Experiment II observed a trapezium window (Day and Power, 1963) shape cut from 1/16 in. aluminium sheet and painted matt white. The parallel edges of the window were 11·73 in. and 6·25 in., whilst the non-parallel edges were 9·625 in.

SUBJECTS

In both Experiments I and II there were 30 subjects from an introductory course in psychology. Subjects for the first experiment were 10 men and 20 women and for the second 15 men and 15 women. In each experiment they were randomly allocated to the two groups. No subject served in both experiments, and none had had prior experience of the phenomenon.

INSTRUCTIONS

Preliminary instructions to both groups

In this experiment you are going to observe an object which is rotating, like this (Experimenter demonstrates with a 6 in. square mounted on a rod). When people look at an object of this type they sometimes see it change direction, like this. That is, some people experience an illusion that the direction of motion changes. I want to find out how often you see this particular object change direction.

Oscillation instructions

I want you to press this button when the object appears to change direction, like this. Remember, I want you to press the button when it seems to change. I am not concerned with what you know or think is happening. Look at the centre of the object rather than at any other part.

Supplementary instructions to the oscillation group

Look at the centre of the object and press the button each time the object seems to change direction.

Rotation instructions

I want you to press this button when the object appears to move from the edge on position around through 180° to edge on again, like this. I am not concerned with what you know or think is happening. Look at the centre of the object rather than at any other part.

Supplementary instructions to the rotation group

Look at the centre of the object and press the button each time the object appears to move from the edge on position around through 180° to edge on again.

PROCEDURE

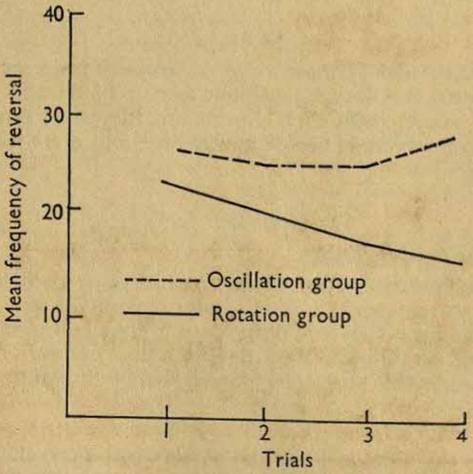
Each subject was given four trials, each of twenty revolutions, with a rest period of 64 sec. between trials. On being seated, the subjects were given the preliminary instructions, then either the oscillation or the rotation instructions. The shutter was opened when the shape was edge on to the subject. After 20 revolutions the shutter was closed and the subject told to rest. At the beginning of the next trial the subject was given the appropriate supplementary instructions. The same procedure was used for the remaining trials. All observations were monocular, the subject using his preferred eye.

It has been found that some subjects report reversals in the sagittal plane (Power, 1964). In the two experiments any subject who reported such reversals was discarded, because whilst subjects in the oscillation groups could signal the occurrence of sagittal reversals, subjects in the rotation groups could not signal the non-occurrence of these reversals. This rejection was biased against the hypothesis because subjects reporting such reversals usually report reversals in the frontal plane.

RESULTS

Scores for the subjects in the oscillation groups were obtained directly from the records, whilst the scores for the subjects in the rotation groups were obtained by substracting the number of times the subject pressed the button from 40, the total number of reversals possible in the frontal plane during 20 revolutions (Day and Power, 1963, 1965). The data from both experiments were subjected to analysis of variance for repeated measures with the same subject (Winer, 1962).

FIGURE 1



Mean frequency of apparent reversal of an elliptical shape reported during four 20 revolution trials in Experiment I.

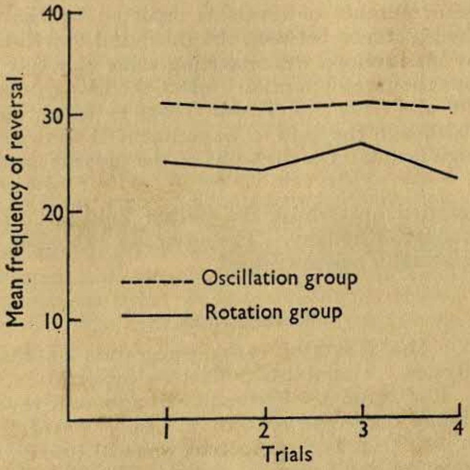
Experiment I. The data from six subjects in the oscillation group who reported reversals in the sagittal plane were discarded for the reason given above and replaced by additional subjects. Mean frequencies of reversal are shown in Figure 1, and a summary of the analysis of variance is given in Table I. The group given the oscillation instructions reported significantly more reversals ($\bar{X} = 26{\cdot}8$) than the group given the rotation instructions ($\bar{X} = 19{\cdot}5$). The trial effect by itself was insignificant, but as the significant interaction shows, the differences between the groups increased significantly over trials, the difference on the first trial being 3·4 reversals, and on the last trial 14·0 reversals. *t* tests for correlated means were carried out on the data from the first and last trials for each group. *t* for the oscillation groups was 1·19 which with *d.f.* = 14 was non-significant ($p > 0{\cdot}1$). *t* for the rotation group was 2·37, which with *d.f.* = 14 was significant ($p < 0{\cdot}025$). A one-tailed test was used in each case since the direction of the difference had been predicted. Finally a *t* test for correlated means was carried out on the results of the third and fourth trials of the oscillation group to see whether the instructions had

TABLE I

SUMMARY OF ANALYSIS OF VARIANCE CARRIED OUT ON THE RESULTS OF EXPERIMENT I

Source	*S.S.*	*d.f.*	*M.S.*	*F*	*Probability*
Between subjects	7852·38	29			
Instructions	1620·68	1	1620·68	7·28	0·02 <0·01
Error within groups	6231·7	28	222·56		
Within subjects	5508·75	90			
Trials	220·69	3	73·56	1·29	>0·05
Trials X Instructions	513·49	3	171·16	3·01	0·05 <0·02
Error within subjects	4774·57	84	56·84		
Total	13361·13	119			

FIGURE 2



Mean frequency of apparent reversal of a trapezium shape reported during four 20 revolution trials in Experiment II.

TABLE II

SUMMARY OF ANALYSIS OF VARIANCE CARRIED OUT ON THE RESULTS OF EXPERIMENT II

Source	*S.S.*	*d.f.*	*M.S.*	*F*	*Probability*
Between subjects	9979·37	29			
Instructions	2726·53	1	2726·53	10·52	0·01 <0·001
Error within groups	7252·84	28	259·03		
Within groups	3099·0	90			
Trials	124·3	3	41·14	1·23	>0·05
Trials X Instructions	171·94	3	57·31	1·72	>0·05
Error within groups	2802·76	84	33·37		
Total	13078·37	119			

had an effect late in the testing session. *t* was 1·898, which with *d.f.* = 14 was significant ($p < 0·05$).

Experiment II. The data from four subjects in the oscillation group were discarded since they reported reversals in the sagittal plane and were accordingly replaced by additional subjects. The mean reversal frequencies are shown in Figure 2 and a summary of the analysis of variance of these data in Table II. The group given the oscillation instructions reported significantly more ($\overline{X} = 34{\cdot}9$) reversals than the group given the oscillation instructions ($\overline{X} = 25{\cdot}4$). The trial effect was not significant, and while the mean difference between the groups was 5·9 on the first trial and 12·1 on the last this increase was not significant.

DISCUSSION

The results of both experiments confirm the hypothesis that instructions to signal reversals lead to a greater number of apparent reversals. Further, these effects occurred when the subjects were not familiar with the intentions of the experimenter (as Mulholland's 1963, subjects were), nor were they under pressure to report certain types of motion (as Cappone, 1963, and McGee's, 1963, subjects were). The subjects seemed to accept the implication from the instructions that the experimenter was concerned to establish the frequency with which the object appeared to reverse.

The significant interaction in Experiment I shows that the effect produced by the instructions increased over time. The *t* values obtained for the differences between the first and fourth trials of the two groups shows that this interaction occurred principally because of the decrease in number of reversals reported by the rotation group. The significant *t* value for the difference between the third and fourth trials of the oscillation group indicates that the instructions were exerting some effect at this stage. It is not clear why the instructions should differentially affect the two groups, and this data is not consistent with earlier results (Day and Power, 1963) in which an increase in reversals was found over time. Although the data in Experiment II show an increased effect over time, this effect is not significant. On the basis of the present data this difference is not readily explicable.

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COMMENTS ON "EXPECTANCY AND INTERMITTENCY"

BY

LAWRENCE KARLIN

From the Psychology Department, New York University

In a recent experiment, Davis (1965) interprets his data to mean that the lengthened reaction times usually obtained in response to the second of a pair of closely spaced stimuli are conclusively due to some sort of central refractoriness produced by the first stimulus. He bases this conclusion on a situation in which the first response is made *ad libitum* without the usual first stimulus while the stimulus for the second response is triggered by the first response after a variable delay. Since he finds that RT shows little change as a function of these delays when there is no first stimulus, while it showed appreciable change when there was a first stimulus (Davis, 1957, 1959), he concludes that lengthened RT's in the two stimulus situation are "due to the effects of the first stimulus and not to the distribution of interstimulus intervals." However, the Davis experiment is not conclusive as he maintains because the nature of preparation as well as the speed with which it develops may depend on more than the quantitative characteristics of a distribution of time intervals, it may depend on the events which bound the intervals as well. For the purpose of preparation the interval between an *ad lib.* response and a subsequent stimulus may not be equivalent to the interval between two stimuli or the interval between a stimulus-elicited response and a subsequent stimulus. Therefore, the omission of a stimulus may not be decisive for either Davis's point of view or one which emphasizes preparation. This ambiguity might be eliminated by a control experiment using the same distribution of intervals in relation to the first response but in which the first response would be made to a stimulus rather than *ad lib.* If RT of the second response varied as a function of interval it would support the present criticism since any prolongation of the second RT for stimuli occurring after the first response could not be readily attributed to the type of refractoriness which Davis hypothesizes. His earlier experiments (1957, 1959) which show a tendency for the second RT to change even when the second stimulus followed the first response suggest the possibility of finding this result.

On the other hand, even if RT did not show any change, it could be argued for both Davis's (1965) experiment as well as the suggested control that a distribution of intervals between first response and subsequent stimulus is not equivalent to the same distribution in which the second stimulus can occur prior to the first response. It could be argued that for the purposes of preparation, the former distribution has a longer mean interval and a somewhat different shape, since the first response time whether made *ad lib.* (as in Davis's experiment) or to a stimulus (as in the suggested control) is added to every interval in the distribution.

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A REPLY TO DR. LAWRENCE KARLIN

BY

R. DAVIS

From the Department of Psychology, University of Reading.

I regarded this experiment as establishing conclusively that the lengthened reaction times usually found in responding to the second of two closely succeeding signals are not due solely to the temporal uncertainty of the second signal with respect to the first. This is not to deny that the temporal uncertainty of the second signal may have *some* influence on the reaction time to it, but merely that temporal uncertainty alone is insufficient to account for the very considerable lengthening of reaction times observed in earlier experiments at intersignal intervals of 200 millisec. or less.

I quite agree with Karlin that it is not merely "the quantitative characteristics of a distribution of time intervals" which are important, but that one must also consider the "events which bound the intervals." Indeed, this was the very point I was trying to make. Furthermore, if he assumes that the arrival of a signal interferes with the "preparation" of a response to a subsequent signal, then his argument as it stands seems very like the intermittency argument stated in slightly different terms.

In the experiment which Karlin suggests it seems that temporal uncertainty would be the main source of variation in reaction times. The magnitude of these variations would depend on the range and distribution of intervals between the first response and the critical signal, but if these were the same as in the "spontaneous response" experiment I would again expect to find almost negligible variations in reaction time with respect to this interval.

With regard to the use of this experiment as a control, I don't see why Karlin considers that the results would provide any more effective control than the results already available from experiments on

(*a*) responding to both of two consecutive signals;

(*b*) responding to the second only of two consecutive signals;

(*c*) a "spontaneous response" followed by a response to a subsequent signal.

Manuscript received 12th June, 1965.

A SIMPLE DEVICE FOR A CONTINUOUS PAPER-CHANGE

BY

J. A. LEONARD and A. CARPENTER

From the Medical Research Council, Applied Psychology Research Unit, Cambridge

Broadbent and Heron (1962) have described an immediate memory task in which subjects have to check through lists of numbers according to various rules. The numbers are tabulated 10 to a row and covered by a sheet of cardboard. A slot in this allows the subject to see only one row at a time, and to mark the paper in pencil. The experimenter has to ensure that the cardboard is only moved according to the instructions.

This task was used by Colquhoun (1962) as a group test, and the device now described was designed to ensure that the rules were adhered to without detailed supervision. The device is simple and cheap enough for a number to be made, and has already proved useful in other applications.

FIGURE 1

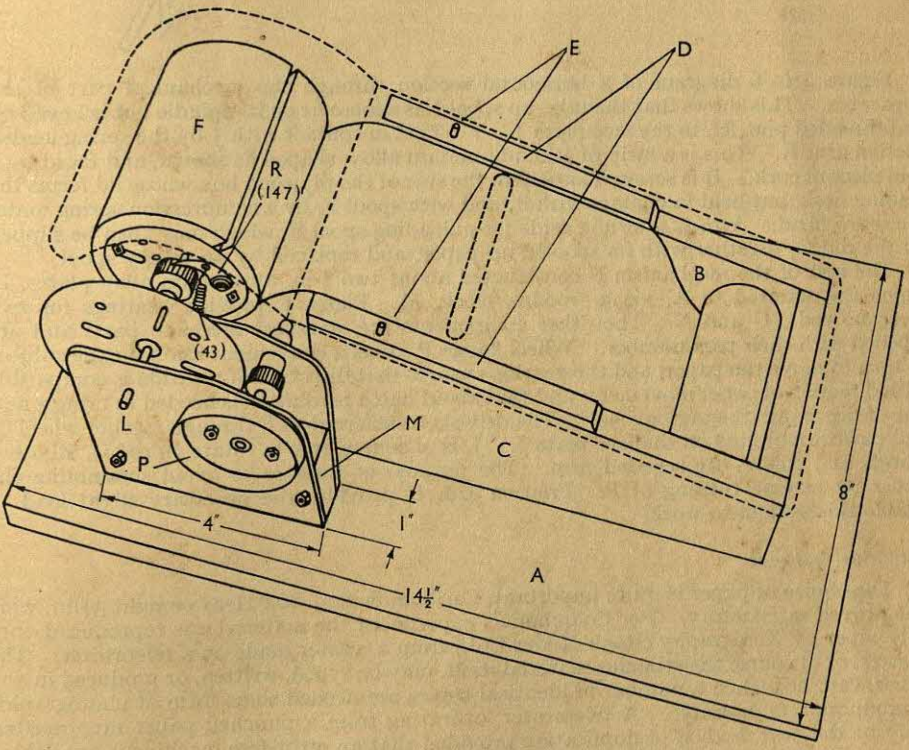

Figure 1 is a line drawing of the apparatus. The baseboard, A, the paper delivery box, B, and the structure supporting the "reading desk," C, are made of $\frac{1}{2}$-in. plywood. From B the paper then passes up the inclined reading desk between two guides, DD, of $\frac{1}{8}$-in. hardboard, and on to the wooden take-up spool F. Throughout this path the paper is hidden by a shaped cover of 18 s.w.g. aluminium sheet, shown dotted in Figure 1, and retained by two 4 BA screws, EE. The slot in this cover, through which the subject inspects, and marks, his digits, is also shown dotted.

FIGURE 2

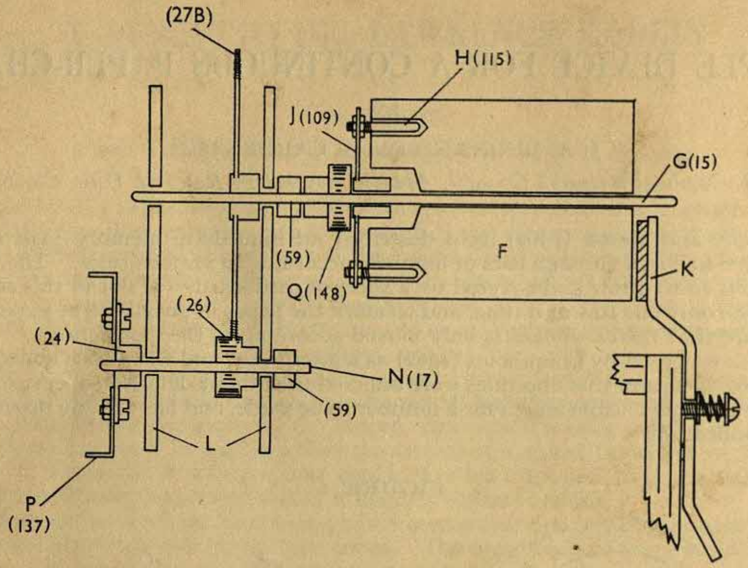

Figure 2 is a diagram of a horizontal section through the mechanical part of the apparatus. This shows that the take-up spool F is a loose fit on its spindle but is keyed by two threaded pins, H, to the face plate J. F is held in contact with J by the spring loaded friction arm K. This is a strip of ⅛-in. aluminium alloy, shaped as shown, and faced by a thin piece of cork. It is screwed loosely to the side of the plywood box whose lid forms the reading desk, but held in contact with it, and with spool F, by a compression spring under the screw head. Arm K is swung aside for unloading spool F, which may then be slipped off the rod G, together with its spooled up paper, and replaced by an empty spool.

The rest of the mechanism is constructed about two ⅛-in. aluminium alloy plates, L, which are screwed to A, via a wooden block, M. Plates L provide bearings for two Meccano rods, G and N. The other components are standard Meccano parts and are labelled with their part number. Wheel flange P forms a convenient knob for the subject to turn to move the paper, and the gearing ensures that direction of rotation is compatible with direction of paper movement, and that about half a revolution is needed to bring a new line of digits into the viewing slot. The drive is transferred to F through ratchet wheel Q, and pawl R mounted on the face plate J. J, is of course, free to rotate on rod G, all other wheels and collars are screwed firm. The purpose of these is to avoid scrambling the paper by reverse turning of P. Friction arm K provides the necessary slight load to enable this system to work.

Stimulus material

The choice of paper is quite important, Caribonum Red Box Heavyweight 3¼ in. wide has proved satisfactory. For Colquhoun's experiment the material was reproduced onto this paper by Xerography (Rank-Xerox Ltd.) from a master made on a teleprinter. The master, or of course the stimulus material itself, may be typed, written, or produced in any other way, but when a number of identical copies are needed some form of photographic reproduction is desirable. A teleprinter, operating from a punched paper tape master, may be the best method of duplication provided that an error-free machine is available.

Applications of this apparatus are quite wide, for example one has been fitted with a driving motor to produce a paced situation (Woodhead, 1964). Again, it has been used as a simple teaching machine (Knight, 1964).

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Manuscript received 19*th February*, 1965.

OBITUARY

KENNETH RONALD LAMBERT HALL (1917–1965)

The unforeseen, bitterly premature death of K. R. L. Hall at 47 deprives his discipline, his University and this Society of a man who could ill be spared and will not soon be replaced. Not only was he an outstandingly successful head of department, but his freedom from the parochial, and readiness to collaborate with workers in neighbouring fields, greatly refreshed our subject.

After a classical upbringing at Cheltenham, the pursuit of law at Oxford brought less distinction than achievements on the hockey field and in the boxing ring. Service with the army from 1940 to 1946 followed and after demobilization he was soon back at Oxford to work for the D.Phil. in the Institute of Experimental Psychology on "The Verbal Factor in Perception," under William Stephenson. Later in the same year his interests reorientated themselves and a stroke of the pen switched his research to "Some Verbal Factors in Perception." Taking up a few of the many issues raised but left open by Bartlett in *Remembering*, he produced an effective study of the relation of words to the structure and meaning of signs. His D.Phil. gained—it was the first to be awarded in psychology by the University of Oxford—he was appointed head of the department of clinical psychology at Barrow Gurney Hospital, and became Lecturer in Clinical Psychology in the University of Bristol. At Barrow his energy and acumen were particularly directed to the introduction of experimental methods into the study of psychiatric illness and he covered a wide range, from learning impairment in schizophrenia to a systematic exploration of conditions affecting pain thresholds. In 1955 he was appointed to the Chair of Psychology at the University of Cape Town where he remained until 1959. It was this period, with its circumstances and opportunities, which brought to flower the passionate interest in animal behaviour which continued throughout the rest of his life. The earlier origins of this are not easily discerned—as a schoolboy and undergraduate it seems to have gone no further than annotations in his bird book. In South Africa systematic observation of behaviour replaced identification. The step from birds to baboons soon followed and resulted in a number of papers on the habits of *Chaema* in its natural habitat.

In 1959 Hall inherited the admirable opportunities created by his predecessor, G. C. Drew, in the Chair of Psychology at Bristol. Here his varied energies and talents found full scope. The department flourished under his skilled management and wide appreciation of the diversified skein of contemporary psychology. Increased academic responsibilities were not to stifle his own research activities, and Hall set about the task of raising the scope and scale of primatological studies in this country to the level they deserve and require. In his own department he installed the facilities and resources needed to introduce—and persuade to breed—fresh primate species as experimental animals. (Less than a fortnight before the beginning of his brief illness, he had the satisfaction of finding that the birth of a Patas monkey had taken place uneventfully during the lunch hour without the mother being segregated from the colony, which included the infant's father and older brother). He made expeditions to various parts of Africa, and, gaining the confidence of those in a position to give financial support, was actively and successfully arranging an exchange of research facilities for graduate students and other workers between this country and a centre in Uganda. He took an active share in primate developments in the United States. Major plans for a substantial increase of research at Bristol were almost realized. All of these things will be the poorer for his death, and some temporarily extinguished.

Hall's presence will not quickly fade in the minds of those who knew him. Thickset in figure and voice, he gave an unusual impression of powerful but collected exuberance. His concentration on whatever he was doing, and his evident enjoyment of it, were irresistible to his companions. He was practical, strongly directed towards his aims and quite undeterred by unfamiliar technicalities, which he easily mastered. A shrewd but gentle sense of humour prevented the purposeful from harbouring the ruthless. His loss as a person is heavy, as the gap he leaves in contemporary British psychology is serious.

R. C. OLDFIELD

CORRESPONDENCE

The Editor,
Quarterly Journal of Experimental Psychology.

DEAR EDITOR,

SIR FREDERIC BARTLETT LECTURESHIP FUND

A number of members have enquired about the finance needed to establish the Sir Frederic Bartlett Lectureship Fund, and it may be a help, both to members and others intending to contribute, to give some idea of the target figure the Society hopes to achieve. The Committee considers that in order to meet expenses and to provide an honorarium for the Lecturer, a minimum of £150 should be available for each Lecture. If the Lectures are to be given biennially, a capital sum in the region of £2,000, suitably invested, would be needed to provide this amount as interest.

Many of those who were close to Bartlett, as colleagues and students, have already shown by their response how much they appreciate this opportunity to express their gratitude. But there is a much wider group, a younger generation working in a variety of fields, in this country and overseas, who have derived inspiration from Bartlett's ideas, and the Fund will also provide an opportunity for them to express their appreciation in a very practical way, by making a contribution, however small.

Contributions, which will be acknowledged, should be made out in the name of "The Sir Frederic Bartlett Lectureship Fund" and sent direct to the President, Experimental Psychology Society, Psychological Laboratory, Downing Street, Cambridge.

Yours sincerely,
R. DAVIS,
Hon. Treasurer, Experimental Psychology Society.

BOOK REVIEWS

Contributions to Mathematical Psychology. Edited by Norman Frederiksen and Harold Gulliksen. New York: Holt, Rinehart and Winston. 1964. Pp. x + 189. $6.50.

This collection of seven papers was given at a conference organized by the Educational Testing Service to mark the dedication of Thurstone Hall. Thurstone Hall was so named in honour of the late Louis Leon Thurstone, and the papers read at its dedication concerned topics in mathematical psychology in which Thurstone had led the way. The speakers were chosen partly because their work reflected Thurstone's influence.

The first paper by Dorothy Adkins is a short biography of Thurstone and is accompanied by a complete bibliography of his publications. Thurstone originally studied engineering and did not turn to psychology until three years after graduation. His first psychological interest was in learning, but his principal researches concerned the development of psychophysical methods and the construction of subjective units of measurement, the measurement of attitudes, and multiple factor analysis. The other six papers in this collection concern topics within these areas of psychology.

Clyde Coombs summarises his Theory of Data. This is a system for classifying and interrelating models of psychological measurement, and it focusses attention on three basic types of measurement. It is expressed in general symbolic terms. It has a certain methodological relevance to psychological studies; this is demonstrated by an analysis of the similarities between eight American psychological journals. It appears that the diverse interests of these eight journals may be represented spatially in two dimensions.

Harold Gulliksen reviews the history of attitude scaling and then reports on a large programme of attitude measurement in several different cultures upon which he was engaged at the time of writing. Four questionnaires were used; they concerned preferences among "Occupations," "Reasons for Work," "Nationalities" and "Goals of Life." Answers to these questionnaires are being analysed to isolate the factors underlying them.

Ledyard Tucker discusses the extension of factor analysis to three-way classifications. His theory is neither simple nor complete, but he does give a procedure by which his model might be used to approximate observed data.

Paul Horst discusses a dilemma in the construction of psychological tests. If a test is to have a high internal consistency, the test items must be of approximately equal difficulty or preference value. But this means that the test has minimal discrimination power. Horst argues that test theory must explicitly incorporate variation in item difficulty or preference value, rather than treat it as an experimental nuisance. He then develops a model in which the matrix of test scores is approximated by a binary simplex matrix. This procedure supposes that the test items are perfectly reliable and homogeneous, and the rank of the binary simplex matrix is equal to the number of distinct difficulty or preference values presented in the test.

Robert Abelson considers how changes in attitude towards some controversial issue may be represented mathematically. He supposes that, when two members of a group meet, the attitude of each is modified in the direction of the other. But this leads to a prediction of universal agreement amongst the group except where the group falls into two parts with no contact between them. Abelson discusses how the original model might be modified to yield a polarisation of attitudes in certain cases. Finally he describes a computer simulation of the model.

J. P. Guilford discusses the relation between creativity and intelligence in terms of a model of the structure of intellect. This model has been developed in the light of factor analytic studies of intellectual processes. Guilford relates his theory to other conceptions of creativity, in particular the idea that creativity is high intelligence, and the work of the psychoanalysts. Finally he mentions some methods for future experimental study of this problem.

These papers are all methodologically oriented and they will interest only the specialist in this field. They concern further refinements and applications of the analytical methods bequeathed by Thurstone, which seem to have generated their own lines of further work untempered by behavioural problems. Whereas Thurstone's methodological studies were initially inspired by particular psychological problems, the work reported here has the feeling of having been begotten of Methodology itself. It may well be asked where this work is leading and whether it will produce anything more than the routine result.

The title of this book might also mislead some. This is the mathematics of psychological tests and factor-analysis, not the mathematics of psychological theory. This distinction and the criticism of the fundamental inspiration of this research may be tersely summarised: here are contributions to psychological mathematics rather than to mathematical psychology.

D. R. J. LAMING.

Experiments in Motivation. Edited by H. J. Eysenck. Oxford: Pergamon Press. 1964. Pp. viii + 424. £5.

This book is mainly made up of reports of separate experiments carried out by Professor Eysenck and his associates. The greater part of it deals with human behaviour, though the last five chapters report experiments on rats. All the experiments are concerned, to a greater or lesser degree, with the role played by motivation in the control of behaviour.

The largest group of results is derived from one experimental situation. Two groups of boys (15–18 years old) served as subjects. The first group (high drive) were taking part in a competitive selection procedure for a particularly attractive apprenticeship scheme; the second group (low drive) were apprentices who had already been selected by the same firm. Both groups were given various tasks (e.g. pursuit rotor, multiple choice reaction, digit span) under similar conditions, except that the high drive group were under the impression that these tasks were part of the selection procedure, while the low drive group knew that this was just an experiment, and that their performance would have no effect on their career. It is argued that this real life situation is a more satisfactory way of studying the effects of drive on behaviour than the more familiar methods of giving "motivating instructions" or measuring drive by means of questionnaires. Indeed both these latter methods are very critically reviewed in chapters of this book. The results of the various tests seem in general to bear out the view that this is an effective way of varying motivational level; for example the high drive group showed more reminiscence with the pursuit rotor task, a higher level of performance in eyeblink conditioning, and greater caution (i.e. slower speed and greater accuracy) in a number of more complex tasks. Of course the two groups differed in other ways than their level of motivation since the low drive group had been selected by the firm. This is acknowledged in the first chapter where it is reported that the low drive group were significantly superior to the high drive group in various types of intelligence test and tests for manual dexterity; but the authors point out that these differences would lead one to expect superior performance on the part of the low drive group, while the differences arising from differences in drive are expected to be in the opposite direction. This difficulty would have been overcome, at the cost of a good deal of extra work, by using only those members of the high drive group who were subsequently accepted by the firm. The authors do not consider this possibility, and in cases where there is a clear cut superiority in the high drive group such a precaution is probably not necessary; but in many of the experiments reported the relationship between the groups is a complex one with no very clear superiority in the high drive group, and in these cases more should surely have been done to control for these differences. This objection becomes particularly strong in those cases in which the Yerkes-Dodson law is used to explain experimental results. For example, in Chapter 8 it is reported that the low drive group show superior performance in a complex multiple-choice reaction task, and this is taken as evidence in favour of the Yerkes-Dodson law; no mention is made of the equally possible explanation in terms of differences of ability, manual or intellectual, between the two groups. This type of omission is made more serious by the authors' use of this difference between groups to explain other results which fail to match their expectations (e.g. the lack of difference between the groups in digit span).

The second part of the book deals with the effect of "motivating" instructions on learning and performance. A lengthy review of the literature and some experiments both lead to the conclusion that this is a highly unsatisfactory way of manipulating motivation. This section also includes a report of some rather complex results on the effects of other concurrent tasks on eye-blink and PGR conditioning in human subjects.

The final section of the book is concerned with the differences between the Maudsley Reactive and Non-reactive strains of rats in various types of situation. The results on the differences in behaviour of these two strains in conflict and frustration situations are very interesting and strongly support the view that these strains do indeed differ in some very general way in their degree of emotionality. This whole important field of work shows that we should take the question of "individual differences" in animals very seriously.

Although the individual experimental results are mostly interesting and worth reporting, the book suffers from a lack of cohesion. Most chapters are essentially experimental reports, suitable for journal publication, with relatively brief discussion sections. These are interspersed with review chapters, but these do not bring the experimental reports together; they are reviews of literature already published and are far too condensed to be informative to anyone who is not already pretty familiar with the subject being discussed. In some cases these reviews seem to have been taken directly from Ph.D. theses and are clearly not well adapted to publication in a book. There is no general discussion of the implications of the results; though the individual discussions are unified to some degree by their use of Hullian concepts, particularly reactive and conditioned inhibition. One is left with the uncomfortable feeling that the most important implications of the results are being missed; some results are in accordance with Professor Eysenck's general position; some are not, but which are important and why? It would surely have been better to have published the experimental results in journals and then to have written a shorter, more coherent and less indigestible book discussing their importance and significance.

For a volume at this price there are far too many small mistakes. Almost every page contains a misprint, some of the figures in the tables appear to be incorrect, and the table headings are sometimes highly confusing. Names are misspelt or worse (in one case Broadbent appears as Broadhurst), in places the bibliography is not in alphabetical order and a number of dates and page references are incorrect. The style of many of the chapters leaves much to be desired and grammatical mistakes are frequent; "to affect" is not, for instance, synonymous with "to effect." This reinforces the impression that the book was rather hastily thrown together; but this is a pity, for the results reported and the methods used are valuable and deserve better presentation.

M. S. Halliday.

Experimental Studies in Verbal versus Figural Learning and Retention. By Lars H. Alin. Gothenburg: Almquist and Wiksell. 1964. Pp. 216. 40 Swedish Kronor.

This monograph is a Ph.D. thesis submitted to Gothenburg University. As is customary in Scandinavian Universities the thesis was required to be published before its examination. And as is customary in Ph.D. theses everywhere it contains two clearly separate parts: a critical review of earlier work, and a series of original studies. The first 94 pages comprise a detailed account and interpretation of experimental studies of verbal learning and retention. The research of Ebbinghaus is treated in extraordinary detail, and the author presents several interesting re-analyses of Ebbinghaus's data. He then meanders through the works of several early continental researchers, and discusses publications no longer readily available. (Particularly appealing is the case of Finkenbinder (1913) who "by careful reasoning based on false premises happened to use the wrong formula.") Modern approaches to interference theory are then dealt with, and the relevance of information theory to learning studies is discussed.

Most of this makes interesting reading, although the author often gives the impression of arguing for the sake of it. Almost inevitably, exception may be taken to a number of his conclusions. And a large proportion of the first part is only very tenuously related to the 14 original studies reported later in the thesis. A number of interesting findings are described here, mainly concerned with long term (up to 206 days) retention of connected verbal material. Three studies using figural material are also reported; but (despite the title) these are not directly contrasted with the verbal studies. The small numbers of subjects employed are often disturbing, and the application of statistical procedures is somewhat erratic.

This monograph raises questions about the value of publishing Ph.D. theses in their original form. It is probable that the requirements of a Ph.D. examiner differ in a number of ways from those of a specialist reader. For example, the examiner may wish to be satisfied that the candidate is familiar with the work of Ebbinghaus, Luh, Tsai, Kruger, etc., but the psychologist requiring details of Alin's research contributions is unlikely to be troubled about this point. In addition to this, the monograph is atrociously published—probably because of the limited time available before the examination date. Subsequent notification is given of some 70 *errata*, but at least as many mistakes remain in the text. Tables and figures are often difficult to understand in the absence of legends; and the typographical standard is far from high. Alin's work is undoubtedly interesting and worthwhile, but it suffers considerably from a lack of constructive editorial assistance.

PETER B. WARR.

Logical Foundations of Psychological Measurement. By Strange Ross. Copenhagen: Munkegaard. 1964. Pp. 152. 36 Dan. kr.

As much writing on measurement, especially psychological measurement, is confusing or obscure, this monograph will serve a useful purpose as a careful exposition of a theory of measurement which derives from the work of Campbell and others, and which the author closely relates to a widely accepted account of scientific theory. He takes a theory to consist of a calculus coordinated to a semantical system, elements of which may refer to certain objects and the relations between them. These objects can be measured—numbers can be assigned to them—if the calculus which applies to them is isomorphic with a logico-mathematical calculus for ordinal or additive measurement. This account is first developed in relation to physical measurement and then extended to psychological scaling. It is careful and systematic, with a number of lengthy examples, presented in a degree of detail which may appeal to the logician more than the psychologist. For the most part the translation from the Danish is commendably clear.

Ross concludes with a trenchant criticism of the position associated with S. S. Stevens. He argues that the "basic empirical operations" which are supposed to define the latter's "interval" and "ratio" scales are not adequately formally characterized, and that they are not used in Stevens' sensory scaling procedures. While these claim to be objective, they can in fact only be understood in terms of invalid "subjectivist" assumptions.

Ross proposes that the different sciences deal with the same objects, and make use of the same calculi, but can be differentiated by the operations performed and the resulting relations found between the objects studied. Thus a physicist may use a balance to determine physical relations between a series of objects which he can then use to construct a physical scale of weight—numbers assigned to the objects. Given the same objects a psychologist might determine psychological relations between them corresponding to the verbal responses of a subject, and assign them numbers constituting a psychological scale of weight. But this seems to overlook that the physicist is primarily concerned with the objects, but the psychologist with the subject. It is more revealing to consider another procedure open to the physicist. He may use the relations between known weights and the corresponding pointer readings on a spring balance to study changes in the length of the spring. Good psychological experimentation is perhaps best distinguished by its preference for a similar procedure, the feature of the subject which determines his judgements corresponding to the length of the spring.

M. Treisman.

An Exploratory Study of the Perception of Causality. By Lambros Houssiadas. British Journal of Psychology Monograph 36. 1964. Pp. 88. 22s. 6d.

This monograph reports an experimental study of certain problems arising from Professor Michotte's studies of phenomenal causality, in particular the impressions of compression and of "pushing with contact" (*effet entraînement*). The author adduces abundant evidence that these impressions are stimulus-bound, and much of the monograph is devoted to establishing the stimulus conditions (sizes, speeds, and kinematic relationships of the objects) most favourable to the impressions.

Dr. Houssiadas' main concern is to show that causal impressions may occur when the conditions required by Michotte's concept of the ampliation of the active object's movement are not met. According to this analysis, the movement of the active object is perceptually transferred to the passive, so that the latter is displaced by the movement of the former. Thus there are two displacements but (phenomenally) only one movement. This analysis, says Houssiadas, requires temporal priority of the active object's movement as a *sine qua non*, otherwise there could be no "generation" of this movement. In Houssiadas' experiments on entraining, however, subjects reported causal impressions when the objects entered into motion simultaneously. Furthermore, these impressions were not transitory, i.e. limited to a brief distance called by Michotte the "radius of action," but lasted as long as the objects were visible.

Houssiadas' objections rest, in the reviewer's opinion, on a misinterpretation. Michotte recognized that there were stimulus conditions yielding causal impressions in the absence of temporal priority of the active object's movement, namely, cases of propulsion. His treatment of propulsion makes it clear that no radius of action is to be expected, as ampliation is continuously renewed. It is true that he believed that such impressions were possible only when the active object's movement was in the nature of a change of form (e.g. the straightening of a drawn bow string) and that his normal techniques were unsuitable for providing these conditions. What Houssiadas has shown is that a slight modification of the stimulus conditions makes it possible to create impressions of propulsion with Michotte-type stimuli. Consequently his results, far from discrediting the concept of ampliation, could have been predicted from Michotte's treatment of ampliation in the case of propulsion.

The modification referred to is a slight gap between the objects. This gap was present also in the experiments on compression, where it prevented the impression from degenerating into that of one object simply covering the other, and Houssiadas reports experiments to establish the optimum separation. This finding that there is an optimum spatial separation parallels Michotte's finding of an optimum *temporal* interval between the two phases of the action in normal launching. Both investigators regard the interval (temporal or spatial) as of minor significance, serving to improve, rather than establish, the impressions, but the differences between their findings on entraining suggest that the gap may be of fundamental importance.

Whilst Dr. Houssiadas' results leave little doubt that the causal impressions studied by Professor Michotte and himself are stimulus-bound, the complexity of the stimuli may make readers suspect that these impressions are artefacts of conditions existing hardly anywhere outside the laboratory.

D. G. Boyle.

Introduction to Motivation. By J. W. Atkinson. New York: van Nostrand. 1964. Pp. xiii + 355. 76s.

Atkinson offers this book as an introductory textbook for undergraduates and graduate students. It provides difficult but rewarding reading for both.

There are three main themes running through the book. The first is that of continuous historical development. The arrangement is chronological and Atkinson's systematic attempt to show the links between successive and different sets of ideas gives an impression of steady growth and advancement rather than one of a series of theories in rivalry. This continuity of development is emphasized by his translation of terminology whereby it can be seen that the differences between theories are sometimes linguistic, sometimes matters of emphasis and only rarely matters of open conflict. The many similarities between the Hullian and Freudian views of motivation exemplify this.

The second theme is a concern for the ultimate objectives of research into motivation. A healthy initial worry about the nature and desiderata of good explanations re-appears at intervals throughout the book. The pursuit of these aims provides a pleasant contrast to the plethora of journal articles with minimal introductions and discussions, with which the psychologist is currently bombarded. The original questions arising from Atkinson's philosophy of science are raised and repeated for each new theoretical approach.

The third theme is the interdependence of the study of individual differences and the experimental analysis of motivation. Atkinson illustrates their complementary nature most clearly with his own work on achievement motivation: the integration of the animal learning and individual differences in anxiety in Hullian theory is less successful, but the task is more difficult. These two aspects of psychology are commonly taught as separate courses and perhaps seen by students as independent of each other, but in the final speculative "whither now?" chapter, Atkinson suggests how comparative psychologists might very usefully exploit individual differences among animals in a manner slightly alien to ther traditional set, while he frequently illustrates how individual differences can be exploited for experimental rather than normative purposes.

The descriptions of the work of James, Freud, Lewin, Tolman, Hull and others are liberally supplemented with apposite quotations which convey the feel of their work as well as its content. There is a heavy emphasis on Lewin and the importance of the Galilean rather than Aristotelian mode of thought. The evolution of S–R theory is well documented, although Atkinson himself is happier with the Expectancy × Value language. Atkinson's inclusion of his own work on achievement motivation with its use of both experimental analysis and individual differences gives a completeness to the work which some other recent texts lack. Neither animals nor men are neglected. The speculative review and prospectus is difficult to evaluate in our present state of knowledge; Atkinson's views on the nature of avoidance motives are new and odd, but their validity is a matter for further research.

In the introduction Atkinson defends his right to be selective, but one regrettable omission is the lack of concern and information about the taxonomy of animal or human motives. Ideally the research worker in this field should be equipped with a justifiable list of types of motives. Atkinson does not discuss why varying lists have persisted nor why satisfactory ways of evaluating such lists remain obscure. Lewin argued that one disadvantage of the Aristotelian approach was its obsession with classification, but a dynamic explanation of motivation is incomplete without a taxonomic structure: a major obstacle confronting any worker in the field must be his doubts as to whether any particular motive he is working with can justify and retain its conceptual status. In the past this issue has been of greater significance with human motivation, but animal psychologists, too, are experiencing difficulty with curiosity, manipulatory behaviour and "contact comfort". This omission seems to be the only obvious weakness.

Like Osgood's wider textbook, this is a valuable and scholarly work with just sufficient personal preference for a particular type of integration to produce a provocative and therefore commendable bias.

W. P. Robinson.

Imprinting and Early Learning. By W. Sluckin. London: Methuen. Manual of Modern Psychology. 1965. Pp. x + 147. 25s.

The concept of imprinting was due to the German Naturalist, Heinroth, and was developed by another pioneer Ethologist, Konrad Lorenz. There were, of course, earlier statements and experiments which contained the germ of the same idea. Those of D. S. Spalding (1873) and William James (1890) are often referred to. However, it was the writing and experiments of Konrad Lorenz which brought the subject to the centre of the ethological-comparative psychological field in the late 40's and there is has remained ever since. Indeed, since the middle 50's a snowball effect has occurred and it is now quite a specialist's job to keep up with the literature. Dr. Sluckin's book is, therefore, very welcome.

The imprinting process, as the term was used in the earlier literature on the subject, was characterized by including four main points or ideas: (i) the sensitive period, (ii) the stability of the learning process, (iii) early learning which later affects many specific reactions such as sexual and social behaviour, as yet undeveloped at the time of learning and (iv) generalized learning. On the whole, subsequent research has tended to emphasize the importance of items (i) and (iv) and to suggest that (ii) and (iii) apply only in rather exceptional cases. It is obvious from this that the term can be used in a considerable variety of connotations and it is one of the few defects of Dr. Sluckin's book that he does not always make as clear as he might, the particular significance of the word as it is employed in different sections of the book. Originally the term had been applied only to attachments rooted in the approach and following responses of goslings, ducklings, chickens and the like. Later reports of apparently similar behaviour of the young of various mammalian species have made it reasonable to use the term much more freely. The author therefore regards the term, "in its empirical sense", as referring to the formation by young precocial birds of relatively specific attachments. But as he shows in later chapters approach and following are not crucial for imprinting; and attachment to any configuration of stimuli may be formed by the organism as a result of *exposure* to the stimuli. He continues "if so, then exposure to any sensory stimulation leading to any kind of tie to this stimulation may be said to be akin to imprinting phenomena." He rightly sees that such a statement is too vague to serve as a sure means of distinguishing between behaviour acquired in an imprinting-like fashion and behaviour otherwise acquired. But much of the value of the book lies in its discussion, wide-ranging and learned, of various examples of learning which, in one way or another, suggest or approximate to the adjustments which appear to occur in the classical imprinting situation. As a result of this survey he sees that many features of "imprinting" now appear less crucial than they did because they are better understood. That is, many examples now seem to be much more like other forms of learning than was previously supposed. On the other hand, as a result of his survey, he concludes that there are aspects where the difference between imprinting-like learning and other learning is now clearer and sharper. This conclusion arises particularly from Dr. Sluckin's own work and from his collaboration with Dr. Salzen, and concerns particularly what has been called "perceptual learning." Briefly, it amounts to the view that discrimination between the familiar and the strange, consequent upon imprinting, is acquired not by conditioning but through perceptual learning. In this connection he brings in the useful term, "exposure learning" due to Drever. The advantage of this is that it refers unambiguously to the perceptual registration by the organism of the environment to which it is exposed; and it thus indicates the familiarization of the organism with its environment. This is perhaps the most important part of the author's theoretical approach in that it relates the imprinting process to what may be called an "exploratory drive." That is, he examines learning in the imprinting situation in the light of the view that sensory stimulation is a primary need. As a result of this need the organism, human or animal, embarks upon a process of organizing its perceptions in what one may call a physiologically and psychologically "economical" manner. Since, however, the author compares and contrasts so effectively learning by reinforcement on the one hand and by exposure on the other, it is rather unfortunate that he does not discuss his conclusions in the light of Sokolov's work on perception in the conditioned reflex experiment and on neuronal models in relation to the orienting reflex. The book, however, is to be recommended as a careful, and critical account of a great volume of recent experimental work which will undoubtedly play an important part in the future development of ethology and comparative psychology. It would have been still better had there been some attempt at a precise and brief summary of the individual chapters and of the concluding viewpoint of the study. Finally, it should be said that much of the strength of the book lies in the way in which it relates the study of animal behaviour to results and conclusions based on the study of human performance.

W. H. THORPE.

Psychodiagnostic Testing: An Empirical Approach. By Molly Harrower. A Monograph in American Lectures in Psychology. Springfield, Illinois: Charles C. Thomas. 1965. Pp xii + 90. $4·75.

This monograph has two main aims. Dr. Harrower has attempted first to classify disturbed patients into "projective types" or class and degree of "mental health potential." secondly, she has completed a follow-up of 622 patients (out of an initial 2131 cases) via their therapists, with reference particularly to the type and length of treatment received, and the extent of the patient's improvement as judged by the therapist.

The tests on which the "mental health potential" was based included the Rorschach, the Bellvue-Weschsler, the Szondi, the Figure Drawing, the Most Unpleasant Concept, a Sentence Completion test and four cards from the T.A.T. On the basis of these test results, Dr. Harrower has evolved two scales of "personality endowment," each consisting of seven "projective types," the Homogeneous Scale (varying from Very Superior to Impoverished) and the Heterogeneous Scale (ranging from Gifted—with Problems to Very Disturbed). She demonstrates some measure of agreement between "mental health potential" and outcome of therapy.

A. W. HEIM.

Organizational Psychology. By Bernard M. Bass. Boston: Allyn and Bacon. 1965. Pp. viii + 459. $8.95.

Bass is a well-known scholar in this field. The book is, however, not an original contribution in the sense of a report on new research findings or a fresh theoretical orientation, but an assembly of results of empirical investigations linked together by a simple commentary. Its primary usefulness will be for administrators or business course students who might wish to find out whether an issue of concern to them has received professional attention and how the results of inquiries have turned out. The book has probably grown mainly out of courses for people with such needs. The commentary is too elementary for British psychology students. Nevertheless, it will have value in a scientific library as a handy reference to the results of nearly 800 relevant studies.

CYRIL SOFER

Mental Testing in Clinical Practice. By Moyra Williams. Oxford: Pergamon Press. 1965. Pp. xix + 177. 21s.

This sensible little book is based on a good knowledge of the literature and a great deal of practical experience. In spite of its brief compass, it says most of what can usefully be said about the assessment, in the clinical setting, of intelligence and personality, language. learning and memory, given the somewhat *ad hoc* methods at present at our disposal. The assessment of perception and orientation receives more summary treatment.

To clinical psychologists working in the adult field, especially those with an interest in what is (crudely and often misleadingly) referred to as "brain damage," this little book can be recommended as a trusty guide. But it gives signs of some hastiness in preparation; the Bibliography, in particular, requires attention.

O. L. ZANGWILL.

Studies on The Aggressive Behaviour of Mice. By Kirsti Lagerspetz. Finnish Academy of Science and Letters, Series B. No. 131. Helsinki. 1964. Pp. 131. 6.60 mk.

This monograph reports the results of various experiments carried out in the course of a programme of research in which mice were selectively bred for aggressiveness. Aggressiveness is defined as "the frequency and intensity of aggressive behaviour" where such behaviour was shown under constant conditions towards another animal of the same species and sex (it is therefore distinguished from "wildness" or "savageness," which depend on the animal's reactions to the experimenter). The degree of aggression was measured on a rating scale derived from that used by Hall and Klein.

Selective breeding was carried out through seven generations and it was found that the two strains differed in aggressiveness from the second generation onwards. Evidently there is a hereditary basis for this trait. However it was also found, by arranging that mice were victorious or defeated in fights, that the aggressive mice could be made more or less aggressive, though the non-agressive mice could only be made less aggressive (by defeat). Some attempt has also been made to determine the nature of the "drive" involved in aggressive behaviour and the incentive value of fighting by the obstruction method. It appears that mice are only inclined to fight if they have been fighting just before and will be more likely to continue a fight if they are winning. These results would appear to be directly generalisable to mankind. Differences in 'emotionality', maze-learning and motor activity are reported between the two selectively bred strains of animal; it is unfortunate that no physiological measures appear to have been used, particularly in view of the known physiological differences between "emotional" and "non-emotional" strains of rats.

There is a very full review of the literature on this and allied subjects, and both this and the experimental results are discussed with great clarity.

M. S. Halliday.

The Psychology of Time. By Paul Fraisse. Translated by Jennifer Leith. London: Eyre and Spottiswoode. 1964. Pp. vi + 343. 30s.

Professor Fraisse has grappled with the sparse and incoherent knowledge which exists on the *Psychology of Time.* This is the first occasion that a large scale review of this complicated subject has been produced. Although it is a useful attempt, it does have severe shortcomings. The most important of these is that Professor Fraisse tends to glide over the difficult problems, which are, of course, exactly the points where the clearest analysis is needed.

The book is divided into three parts. The first, "Conditioning to Change," asserts that our awareness to time is primarily developed through adaptation to rhythmic changes in the environment. As behaviour becomes linked to these cyclic variations, so the discrimination of successive events and of durations of different length is learnt. It is typical that this idea, which might be acceptable, is justified by a flimsy argument: rhythmic patterns of behaviour must be learnt because they are not shown at birth. While Professor Fraisse tells us that new-born children do not show a diurnal rhythm or sleep, he fails to add that premature babies take longer to develop this rhythm than normal children. But Professor Fraisse has obviously decided that "the induction of organic rhythms from cosmic rhythms" is achieved through learning, and so we are rushed on to experiments on delayed reaction. But there is not a moment's pause to stop and clarify the link between, say, lunar rhythms and delayed salivation. However, an important question does appear: what determines the length of the delay during which a response is inhibited? Where we might have hoped for a clear discussion of the possible time bases to which this delay might be geared, all we are told is that the delay is controlled by a "combination of physiological processes and cues provided by emotion or by action." What does this explain?

The second part of the book deals with the "Perception of Time." This discusses the "Psychological Present," "The Threshold of Time" (particularly concerned with a comparison of the absolute thresholds found for the different sense modalities) and "Perceived Duration" (where the importance of the attention paid to an interval is stressed). It is interesting here to see how the Interval of Indifference (that old bone of time perception theorists) is treated. Professor Fraisse's explanation is that the begining and end of an interval lead to two separate physiological processes which each last about 0·7 sec. Thus for intervals less than this duration, the processes overlap, and for longer ones they are separated. The overestimation of short intervals and the underestimation of long ones is then due to the "effort" involved in "distinguishing" or "associating" the two processes. To add weight to this argument, we are told this interval corresponds to Wundt's optimum time for associations to be made, to the cycle time of the heart beat and to the duration of one step in walking. It is probably a good thing for Professor Fraisse that the evidence now indicates that the interval of Indifference is an artifact; otherwise his idea of "effort" might have proved embarrassing.

The last section of the book deals with our "Control over Time." Here the topics are "The Temporal Horizon" (the past, present and future orientations of behaviour), "The Estimation of Time" (the effect of factors such as age and the intervening task on the estimation of intervals), and lastly "The Notion of Time" (primarily concerned with the development of our rational ideas of time).

The major concept, which Professor Fraisse is continually invoking, is that of change. A further example of the general lack of clarity in this book might be the handling of this important concept. Variations in rates of change may well be responsible for the inconstant nature of psychological time, but we must ask what are these rates of change? Do such widely differing functions as diurnal rhythms and perceptual sampling work off the same time base? If not, are they affected by different factors? Is there any evidence for a particular part of the brain acting as a clock, or do all areas have potential time-keeping mechanisms? Throughout, the reader is left to ask the most pressing questions himself, with little indication of Professor Fraisse's own more precise thoughts.

This is undoubtedly a useful reference book, but it does not stimulate one with critical discussion or interesting prediction. The bibliography is wide-ranging and the translation retains much of the flavour of the original French edition.

C. Legg.

The Body Percept. Edited by Seymour Wapner and Heinz Werner. New York: Random House. 1965. Pp. x + 112. $1.65.

This little book grew out of a Symposium held at the XVII International Congress of Psychology at Washington two years ago under the Chairmanship of the late Dr. Heinz Werner. Its theme was, broadly, the self-perception of the body and the disturbances to which it is subject in abnormal states. Werner and Wapner describe experiments on body-tilt and make some interesting observations on the ways in which perception of the body is affected by the spatial setting. Witkin discusses the development of the body concept, but seems more concerned with individual differences than with general principles. Cleveland and Fisher write on personality factors in perception of the body, while MacDonald Critchley contributes an elegant review of disorders of the body image associated with disease of the parietal lobes. Ajuriaguerra, of Geneva, draws the threads together in an able concluding discussion.

It may be hoped that this book really will attract a little notice. Oddly, perhaps, most psychologists take perception of the body for granted much as laymen take for granted the perception of the external world. How many of us have adapted Koffka's famous question: "Why do things look as they do?" to the perception of our own bodies? Although the studies described in this book are only a beginning (and although not all of them begin at the beginning) they do at least reflect an appreciation of the problem. For without a great deal more basic analysis of the factors that enter into perception of the body, vague notions such as that of the "body-image" will persist in neurology and deter the precise-minded from giving serious attention to the fascinating disorders of corporeal awareness which the term conjures up.

It is perhaps permissible to pay a brief tribute to the late Dr. Heinz Werner, whose death was announced while this book was in press. His work, which extended into a variety of fields, was not as well known in this country as it deserved to be, perhaps because of the *Gestalt* influence which was so marked in his thinking. None the less, Dr. Werner combined a very real experimental interest with a deep concern for the broader issues of mental structure and development. Such a combination is becoming only too rare today.

O. L. ZANGWILL.

Social Psychology in Treating Mental Illness: An Experimental Approach. Edited by George W. Fairweather. London and New York: Wiley. 1964. Pp. xii + 300. 60s.

Much of the recent psychological study of psychiatric hospital wards has tried to identify the characteristics which appear to potentiate what has been called the patient's "career."

Fairweather and his colleagues feel that we now know enough to create experimental milieux, and this book is the full and somewhat overly scientific report of their efforts to do so. They constructed (in the social sense) two wards which were identical in terms of numbers, diagnoses, and staffing, but sharply different in social structure. The experimental ward was designed to foster the growth of peer relationships between the patients by using task groups whose members were expected to take responsibility for each other. There was also a hierarchy of performance levels through which patients had to progress before they could be discharged. The control ward was traditionally run in all respects. The final scientific touch was that half-way through the experimental 27 weeks, the entire ward staffs were changed over.

The upshot of the experiment was that on most of the very detailed criteria of attitudes and behaviour, the patient-centred ward was more effective than the counterpart ward. But although the patients showed striking capacity as responsible, capable people, their subsequent adjustment after discharge was only marginally better.

The differences between the American and British scene are most striking, particularly the isolation of the patients from their families, the lack of any sort of community care, and the very different role of the hospital psychologist. These do not, however, invalidate the general interest of this study for work in this country and it is well worth reading within its context. We have certainly come a long way since Stanton and Schwartz's classic work in 1954.

DOUGLAS HOOPER.

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